SCIENCE

25 November 1960

Vol. 132, No. 3439

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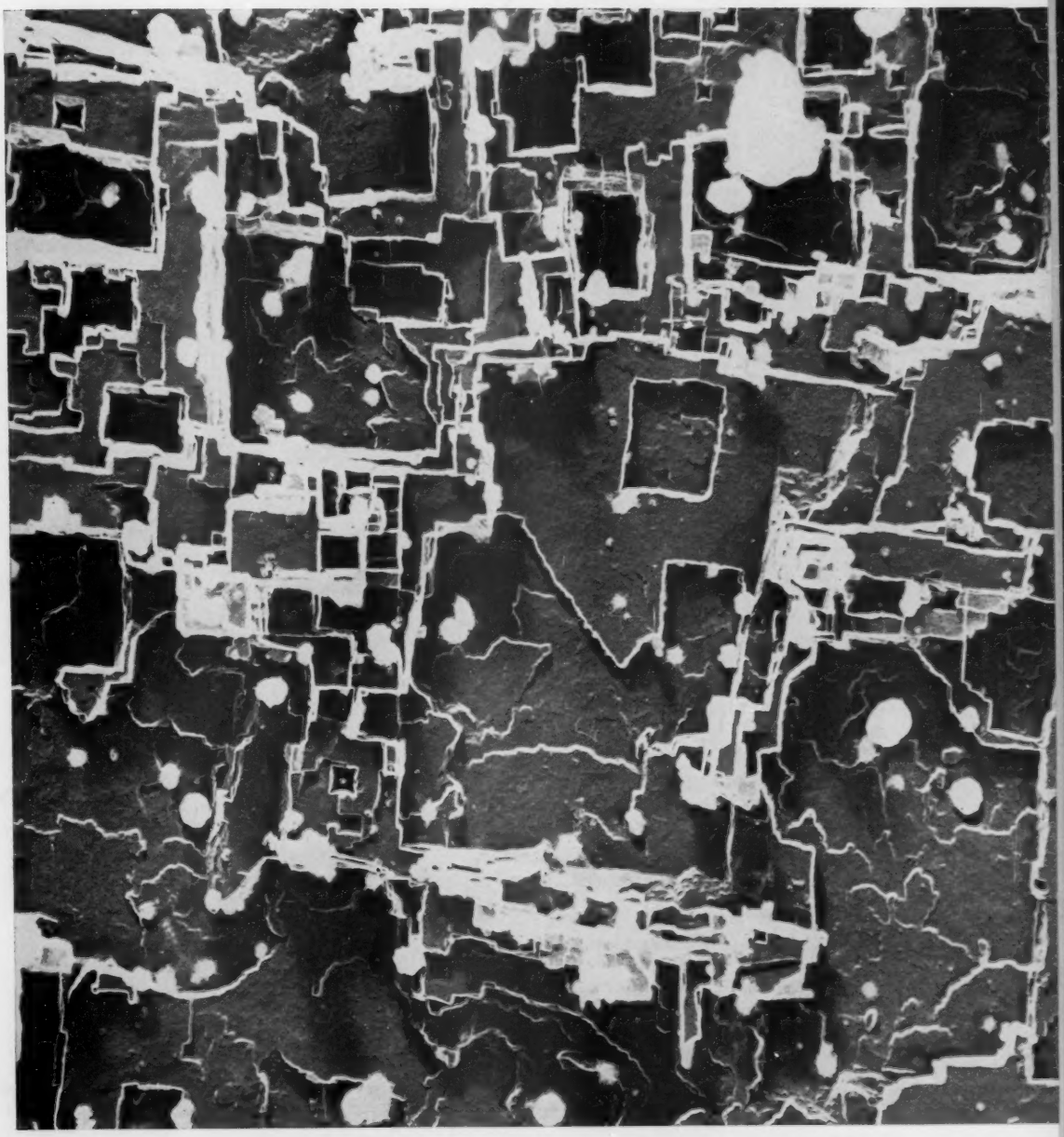

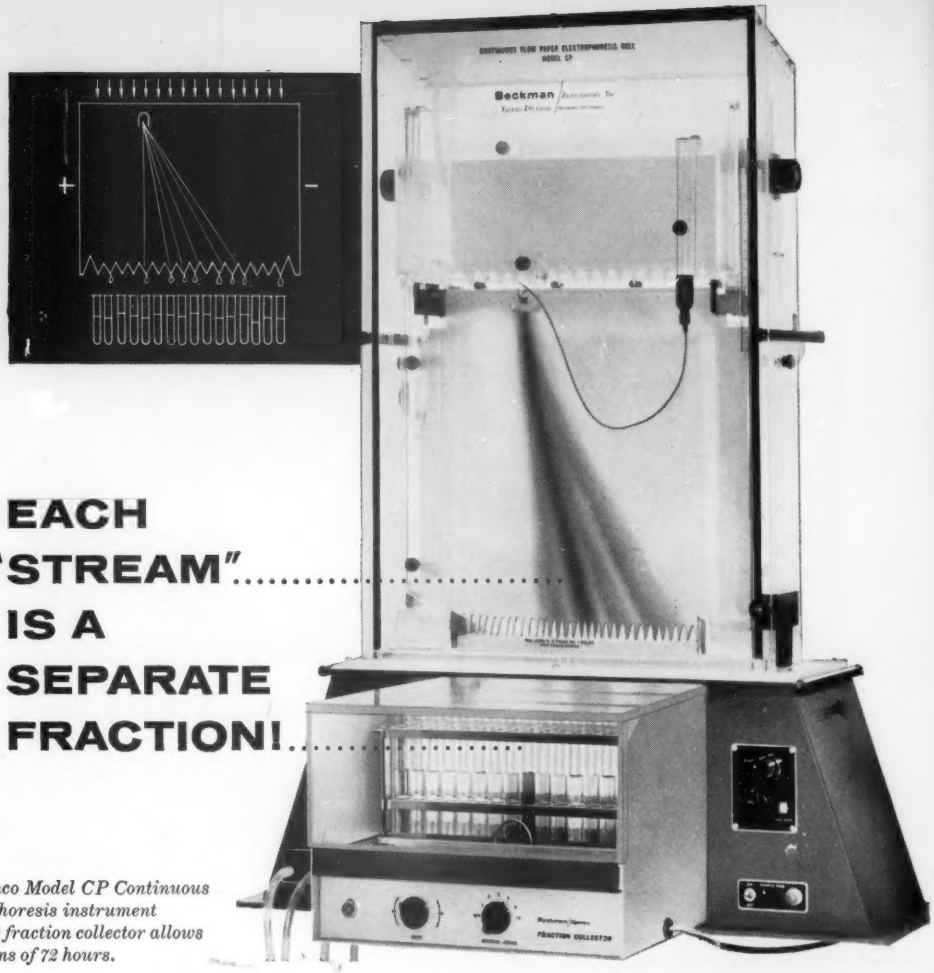

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*American Journal of Clinical Pathology Vol. 33. No. 2. February 1960, pp 144-151 "Application of Refrigerated Microtome in Surgical Pathology" by Bernard Klionsky, M.D. and Othello D. Smith, M.D.

The Journal of Histochemistry and Cytochemistry Vol. 8, No. 5. September, 1960, pp 310 "A Frozen Section Freeze Substitutions Technique and an Improved Cryostat" by Jeffrey P. Chang and Samuel H. Hori.

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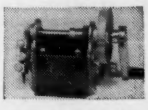

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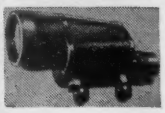

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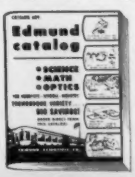

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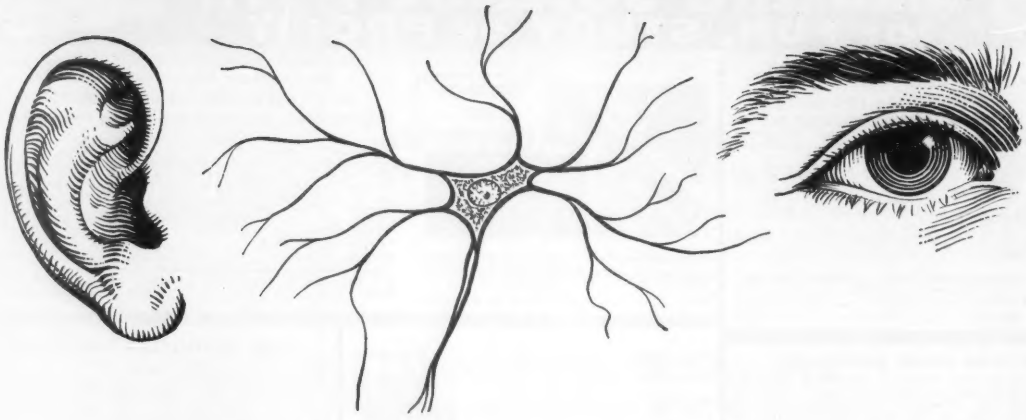


Bell Telephone Laboratories' new electronic "nerve cell" is a step toward finding out

One fascinating area of communications has long resisted exploration—what happens inside the nervous system when you see, or when you hear.

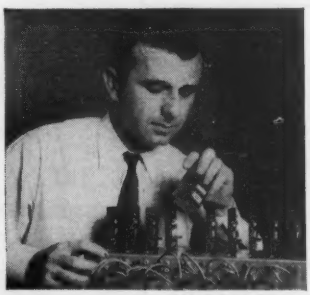
This area is of special interest to telephone science; knowledge of how the nervous system handles sound and picture signals can help determine what information is essential to perception. This in turn may lead to more efficient communication instruments and systems.

To probe the mystery of nerve activity, Bell Telephone Laboratories scientists have developed an electronic model of a living nerve cell or neuron. Consisting of transistors, resistors, capacitors and diodes, the "artificial neuron" exhibits many of the characteristics of a living neuron; for instance, "all-or-none" response and fatigue.

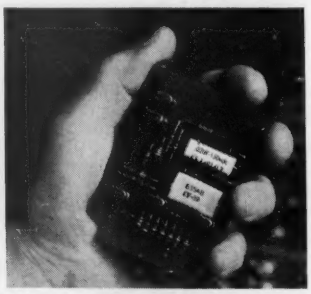
In one experiment at Bell Laboratories, a network of artificial neurons is subjected to a stimulus from light through a set of photocells. The network can distinguish specific patterns of light and dark, thus duplicating roughly some of the eye's basic reactions to light. Similar studies are underway to explore our hearing processes.

At present, too little is known about neural action to permit exact electronic duplication. But experiments with artificial neurons can provide suggestive clues, contributing to a stimulating interplay between electronics and neurophysiology which may help workers in both disciplines.

The human nervous system, including the brain, is the most efficient and versatile data processing system known; and data processing is an essential part of communications. The artificial neuron provides a new approach to investigating and understanding basic nerve network functions. It is a fresh example of how Bell Telephone Laboratories constantly explores new frontiers to improve America's communications system, now and in the years ahead.



Network of neurons is assembled by L. D. Harmon of Bell Laboratories, the initiator of this new research. Many kinds of assemblies are possible.



A single artificial neuron. It delivers electrical impulses when stimulated, like a living cell. Neurons are also being used for research into hearing.

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The Attractiveness of Dessert

It is well known that affluent uncles dote on buying candy and ice cream for their little nephews and nieces. Much less often, however, are these same kindly relatives concerned about paying the grocery bills. And they often lose all interest when the charming little children start to grow up.

The sources which support science—private foundations, federal agencies, and individuals—have a tendency to behave a little like rich uncles. They rather like to give funds for showy pieces of apparatus. They prefer to support the early, pioneering, "demonstrative" stages of a scientific development; following which "someone else" is supposed to take over the less glamorous task of providing support through the pimply stages of adolescence, to say nothing of the dull stages of maturity. Concerning "overhead" the supporters tend to be embarrassed, or bored, or totally uninterested. Equipment and consumable research supplies command high respect. Research assistants, or even research assistance, is fun and fine. Travel and publication costs are often treated as rather questionable items. Heat, light, janitor service, secretarial service, etc.—these after all should be provided by "the institution itself" out of its "own funds," as though colleges and universities kept printing presses in the basement.

When a university genuinely wants to undertake some activity, it impressively confirms their desire if they *do* themselves contribute. And private foundations have, in my judgment, a right to choose projects in which they are partners in support, rather than full supporters.

But an agency, private or governmental, that wishes to aid the support in any field should do so by removing, or at least helping to remove, the *limitations that hamper progress*. If this requires equipment or research manpower or relief from other duties—fine. If it requires a secretary, or travel, or books—fine again. If the institutions in question cannot reasonably meet the increased basic costs of housekeeping, then these should be paid. If remodeled rooms, or a new wing, or a new building is essential, then these often despised "bricks and mortar" necessities are just as sensible and worthy as is any other part of the whole project.

If everybody says that certain costs must be met by somebody else, then who is left over to be that somebody else? And if a pump is worth priming, isn't it pretty sensible to pay the person who goes on pumping?—WARREN WEAVER, *Alfred P. Sloan Foundation, New York*

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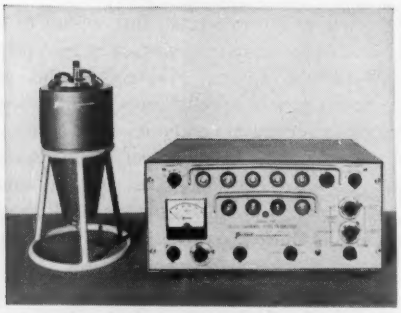

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25 November 1960, Volume 132, Number 3439 SCIENCE

Atmospheric Transport of Artificial Radioactivity

Isotope-ratio and tungsten-185 data show large behavior differences for various stratospheric sources.

E. A. Martell and P. J. Drevinsky

The atmospheric aspects of world-wide fallout from nuclear-weapon tests have been much studied and discussed (*1–6*), and many details of the physical behavior of fallout are now reasonably well known. Nevertheless, there remains a general lack of understanding of large-scale atmospheric transport and mixing processes. Widely different views have been offered to explain the marked spring increase in fallout rate which has been observed at some Northern Hemisphere locations every year over the period 1955 to 1959. The explanation for the excessive accumulation of fallout in north temperate latitudes also is confused. The confusion is due partly to the unsatisfactory nature or quality of much of the fallout data, but more particularly to the use of invalid assumptions with respect to the origin or atmospheric behavior of the fallout debris.

Radioactive fallout usually is classed as local, tropospheric, or stratospheric. Local fallout is radioactive debris which is deposited within a few hundred miles of the test site and is comprised principally of large particles from surface or subsurface nuclear explosions. Tropospheric fallout is nuclear-test debris which is confined to the lower atmosphere below the tropopause throughout its mixing history. Stratospheric fallout is that component of world-wide fallout derived from radioactive clouds which initially rose to heights above the tropopause. For surface or near-surface explosions, tropopause penetration occurs when the total energy yield exceeds about 200 kilotons for the equatorial tropopause and about 100 kilotons for the polar tropopause. In world-wide fallout, it is well known that most long-lived radioisotopes are of stratospheric origin. It usually has been assumed that most short-lived radioisotopes in fallout over areas remote from nuclear-test sites are of tropospheric origin.

Stewart, Crooks, and Fisher (*7*) estimated the mean atmospheric residence time of tropospheric fallout to be about 1 month. After the high-yield U.S. Castle tests in early 1954, Libby (*1*) first pointed out the long holdup of strontium-90 in the stratosphere and estimated the mean stratospheric storage time to be 5 to 10 years. At that time the spring increases and north temperate latitude peak in Sr^{90} fallout were considered to be due to tropospheric fallout from Nevada and Soviet tests. Stewart *et al.* (*3*) attempted to distinguish the tropospheric and stratospheric components of Sr^{90} fallout by use of Sr^{89} and Sr^{90} concentration data for rainfall at Milford Haven, Wales. Taking 35 days as the mean tropospheric storage time and assuming that all the Sr^{89} was tropospheric in origin, they showed that most of the Sr^{90} fallout after 1954 was of stratospheric origin. They attributed the spring increases in fallout rate to a more rapid downward mixing of stratospheric air during the spring, and they related the north temperate latitude peak in Sr^{90} fallout to a selective zone of downward mixing of stratospheric air at middle latitudes. Machta and List (*8*) offer a similar interpretation of the seasonal and latitudinal pattern of fallout. Kuroda (*9*, *10*) has derived a general equation for distinguishing the stratospheric and tropospheric contributions of fallout from isotope-ratio data. The incorrect assumption of a rapidly and uniformly mixed stratospheric reservoir is inherent in his analysis scheme.

The principal shortcoming common to these attempts to interpret the atmospheric behavior of fallout is the failure to distinguish the influence of latitude, altitude, and time of injection on the storage time and fallout pattern for stratospheric sources. In most instances, arbitrary assumptions with respect to the tropospheric fallout contribution also have given rise to confusion. On the basis of evidence provided by fission-product concentration ratios in precipitation, Martell (*4*) pointed out that the short-lived radioisotopes in world-wide fallout were principally of stratospheric origin and that stratospheric storage times decrease markedly with latitude and increase with altitude.

In the present article we use 12.8-day barium-140, 53-day strontium-89, 28-year strontium-90, and 74-day tungsten-185 radioactivity concentrations in individual precipitation samples as a basis for interpreting some of the major features of the transport and deposition patterns for stratospheric fallout. The measurement of the concentration in rainfall of two or more fission-product radioisotopes makes it possible to relate the fallout which is being deposited on the earth's surface to known atmospheric sources of different age, strength, and location in space. It is well established (*1–4*) that the scavenging action

The authors are members of the Geophysics Research Directorate, Air Force Research Division, Bedford, Mass.

of precipitation is the principal mechanism for the removal of radioactive particles from the atmosphere. Thus, the concentration of a given radioisotope in rains and the rainfall records provide a reasonably accurate measure of the accumulation of the radioisotope in soils and surface waters.

The usefulness of data on fission-product ratios as a means of estimating the relative contribution of various nuclear-test sources to total fallout has been questioned on the basis of possibly serious physical and chemical fractionation effects and of uncertainties in fission-product production-yield ratios. For the high-yield nuclear explosions which have been dominant as sources of short-lived as well as long-lived radioisotopes in world-wide fallout, the relatively late condensation times minimize fractionation effects due to delayed formation of radioisotopes with short-lived krypton and xenon precursors. The chemical similarity of barium and strontium limits chemical fractionation effects on Ba^{140}/Sr^{90} ratio data; Sr^{89}/Sr^{90} ratio data obviously are unaffected by chemical factors. For the major sources of fission products in world-wide fallout, the principal production reactions are undoubtedly 14-Mev neutron fission of uranium-238 and fast-neutron fission of plutonium-239 and uranium-235. On the basis of fission yields reported by Katcoff (*11*), calculated production activity ratios of 170 for Sr^{89}/Sr^{90} and 1200 for Ba^{140}/Sr^{90} are unlikely to be in error by more than 20 percent and 40 percent, respectively. Uncertainties due to the use of these production-ratio values, or due to the assumption that there are no fractionation effects, do not seriously affect the usefulness of isotope-ratio data for distinguishing the major differences in atmospheric behavior of nuclear-cloud sources of different altitude and latitude for well-spaced nuclear tests.

A useful tracer for studying the behavior of fallout debris from equatorial tests is provided by the tungsten-185 radioisotope. This radioactivity was produced in a number of surface nuclear explosions in the U.S. Hardtack nuclear-test series at 11° north latitude during the period May through July 1958. Libby (*12*) has estimated the total Hardtack production of W^{185} as 250 megacuries at a reference date of 1 August 1958. Since most of the W^{185} was produced in several surface shots of sufficient yield to thrust the activity into the lower stratosphere, the atmospheric mixing and world-wide fallout patterns for the W^{185} should be characteristic of sources at lower levels of the equatorial stratosphere. Data on W^{185} concentration for rainfall samples from both hemispheres are presented and discussed below.

Experimental Procedures

Individual rains were collected during 1958 and 1959 in a Lucite funnel fitted to a polyethylene bottle at selected sites in both hemispheres. Concentrations of selected radioisotopes are reported for Burlington, Vermont (44°N, 73°W); Bedford, Massachusetts (42°N, 71°W); and Pôrto Alegre, Brazil 30°S, 51°W). The particulate matter of each sample was filtered off and rinsed with dilute hydrochloric acid, with additional water and dilute ammonium hydroxide rinses when W^{185} was to be determined. The liquid fraction of each sample was combined with the rinse solutions, concentrated by evaporation, and analyzed for Sr^{89}, Sr^{90}, Ba^{140}, and W^{185} by standard radiochemical techniques (*13–15*). Analysis of solid fractions of samples was discontinued after early measurements indicated that these contained a negligible fraction of the total activity of each radioisotope being measured.

To samples analyzed for Ba^{140} and Sr^{90}, fuming nitric acid was added to precipitate the combined nitrates. Several ferric hydroxide scavengings were followed by a separation of barium from strontium by the precipitation of barium chromate. Barium was purified by repeated chloride precipitations and ferric hydroxide scavengings. The final sample was barium chloride monohydrate. Strontium was purified by barium chromate and ferric hydroxide scavengings and precipitated as the carbonate, which was milked for 64.6-hour yttrium-90 after Sr^{90}-Y^{90} equilibrium was attained. Yttrium phosphate precipitations were followed by an oxalate precipitation. The final sample was yttrium oxide.

When samples were analyzed sequentially for W^{185}, Sr^{89}, and Sr^{90}, tungsten was separated from strontium by the precipitation of tungstic oxide with hydrochloric acid. Tungsten was purified by sodium carbonate fusion and reprecipitated as the oxide from the aqueous leach solution. The final sample was tungsten 8-hydroxyquinolate. Strontium was precipitated as the carbonate and purified by repeated nitrate precipitations and ferric hydroxide scavengings. Barium chromate and ferric hydroxide scavengings were followed by the precipitation of strontium carbonate, which was counted for total strontium. When secular equilibrium was attained, strontium carbonate was dissolved in dilute hydrochloric acid and Y^{90} was milked by precipitations of yttrium hydroxide and yttrium oxalate. The final sample was yttrium oxide.

All beta measurements were carried out by means of thin-wall, cylindrical, flow-type Geiger counters, of 1.4-centimeter diameter and 3.7-centimeter active length. Details of construction and operation of counters of this type have been given elsewhere (*14–16*). Use of massive-steel and anticoincidence shielding provided background count rates of about 0.2 count per minute. Radioisotope disintegration rates were determined by means of the thick-solid sample technique of Suttle and Libby (*17*). Observed activities were corrected for counter background, sample self-absorption, external absorption, counter geometry, chemical yield, and sample volume to obtain specific activities in disintegrations per minute per liter.

Barium-140 to Strontium-90 Ratios

The Ba^{140}/Sr^{90} activity ratios for individual rainfall samples collected at Bedford, Massachusetts, in 1958 and early 1959 are presented in Fig. 1. A limited number of similar data obtained by Kuroda (*9*) at Lemont, Illinois, and by Suess (*18*) at La Jolla, California, during early 1958 are included for comparison. The Ba^{140} activity data were corrected for decay to the mean time of precipitation. Estimated errors in isotope ratios for Bedford are shown in Fig. 1; these are based on counting errors and decay-curve resolution errors in determining Ba^{140} and Sr^{90} specific activities. The data which show errors exceeding 10 percent represent either very small samples from light rains or samples of very low Ba^{140} activity.

The curve for Ba^{140}/Sr^{90} activity-ratio data in Fig. 1 displays several instructive features. Unlike data for concentration in rainfall of individual radioisotopes, which are very sensitive to rainfall intensity and scatter widely from rain to rain, the isotope-ratio values change smoothly with time. The 12.8-day half-life of the Ba^{140} activity makes the ratio a sensitive indicator of each new test source. The source of

the Ba^{140} component for any data point can be approximately dated by extrapolating back along a Ba^{140} decay-slope line to the intercept date corresponding to the production ratio. Less than a one-week uncertainty in dating is introduced by using a production ratio of 1200. Because of the presence of some Sr^{90} from older stratospheric sources, the actual production date for the Ba^{140} component will be more recent than the intercept date. It is clear that the Ba^{140} activity in rain in north temperate latitudes during March to May 1958 came from the February–March 1958 Soviet tests; that during July to September 1958, from the May–July 1958 U.S. Hardtack tests; and that during October 1958 to March 1959, from the October 1958 Soviet tests. The corresponding Sr^{90} contribution of each test series can be only crudely estimated from the production ratio. However, Sr^{89}/Sr^{90} activity-ratio data for well-spaced tests would provide a more reliable estimation of the Sr^{90} contribution of various tests because the production ratio is more reliable and the Sr^{89} half-life of 53 days is comparable to the durations of nuclear-test series.

It usually has been assumed that the short-lived fission-product activity observed in rainfall and surface air is tropospheric contamination from small weapon tests and is washed out of the troposphere by rains with a half-time of three or four weeks. The data on Ba^{140} and Sr^{90} concentration support the opposite conclusion, that Ba^{140} and other short-lived radioisotopes in fallout are primarily of stratospheric origin. During the periods from mid-April to the end of May 1958, July through September 1958, and January through March 1959, the Ba^{140}/Sr^{90} activity ratio (Fig. 1) decreased according to the decay rate of Ba^{140}. Within each of these periods the average concentration of Sr^{90} activity in rainfall and air either increased with time or remained approximately constant. The Ba^{140} activity, corrected for decay, varied in the same manner. It is clear that the observed Sr^{90} fallout is principally of stratospheric origin. Thus, for these periods it also must be concluded that even the short-lived Ba^{140} activity is of stratospheric origin. A stratospheric origin for both Ba^{140} and Sr^{90}, even during early April 1958 when the decrease in the Ba^{140}/Sr^{90} ratio was more rapid than the decay of Ba^{140}, is plausible on the basis of differences in initial arrival times for debris from stratospheric sources of different altitude or latitude. The rapid decrease in the Ba^{140}/Sr^{90} ratio at mid-June 1958 is explained on a different basis below. The stratospheric origin of the Ba^{140} activity is consistent with both the magnitude of the Ba^{140}/Sr^{90} activity ratios and their gradual and systematic change with time (Fig. 1). For random mixtures of tropospheric and stratospheric sources, one would expect large fluctuations in the Ba^{140}/Sr^{90} ratios and occasional values indicating an apparent age of less than one month. It appears necessary to conclude that most of the short-lived and essentially all of the long-lived radioisotopes in world-wide fallout are of stratospheric origin.

The data on the Ba^{140}/Sr^{90} activity-ratio (Fig. 1) for the period July through September 1958 appeared to indicate an alternation of contribution

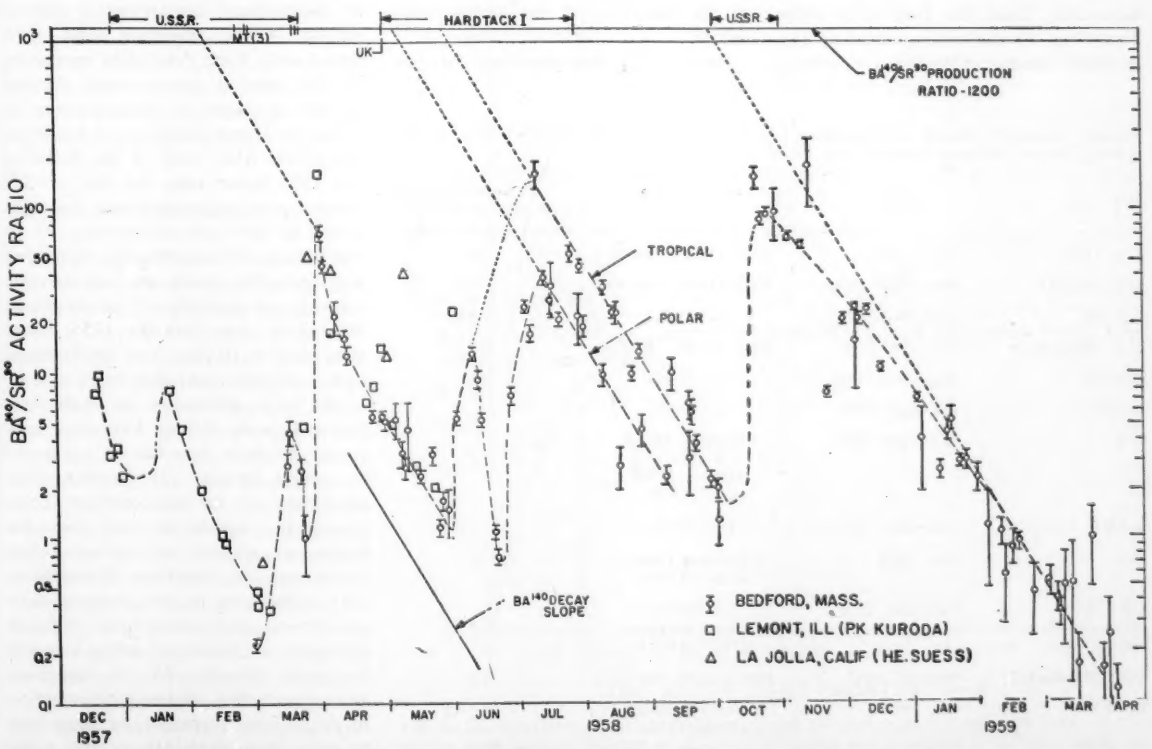

Fig. 1. Ba^{140}/Sr^{90} activity ratios for individual rains collected at Bedford, Massachusetts. The Ba^{140} activities are corrected for decay to the mean time of precipitation. Ratios for a limited number of rains collected at Lemont, Ill. (*9*), and La Jolla, Calif. (*18*), are also included. Test sources for Ba^{140} are indicated. Ratios for July to September 1958 are grouped into two classes, according to the association of a given rain with an air mass of tropical or polar origin.

from two regions of the stratosphere, one containing debris from an early portion of the U.S. Hardtack nuclear-test series and the other containing fresher debris produced several weeks later. This possibility was checked by plotting, at 6-hour intervals, the mean trajectories at several levels (5000, 10,000, and 14,000 feet) for air associated with each rain during this period. Air-mass trajectories were followed for periods of as long as two weeks preceding a given rain in order to establish the origin of the air masses associated with that rain. Without exception, the trajectory data indicated a polar air source for the older debris and a tropical air source for the fresher debris. The persistence of the distinction for nearly three months demonstrates that early Hardtack debris must have mixed to arctic latitudes and that later Hardtack debris was restricted to lower latitudes. It also indicates that mixing of particulate debris across the polar front is slow as compared to the washout rate.

Analysis of the trajectories of air masses associated with individual rains for other periods is helpful in clarifying additional features of the isotope-ratio data. Thus, the June 1958 ratio data (Fig. 1) can be explained as (i) an initial rise due to the first appearance of Hardtack test activity in the tropical troposphere; (ii) a marked decrease in the Ba^{140}/Sr^{90} ratio during mid-June, due to a transition to rains associated with polar air; and (iii) a subsequent rise due to the arrival of Hardtack test activity in the polar troposphere.

Strontium-89 to Strontium-90 Ratios

The Sr^{89} activity in rainfall during periods of well-spaced nuclear tests provides the best basis for estimating the Sr^{90} contribution of the most recent series of tests to the total measured Sr^{90} activity. The accuracy of such estimates is limited by uncertainties in production dates and fission production ratios, errors in radiochemical analysis, and possible fractionation effects. With measurements of good quality for periods of well-spaced, high-yield nuclear tests, however, it is unlikely that the combined uncertainties will exceed a factor of 2. It is shown below that an uncertainty of this magnitude does not seriously affect the usefulness of data on Sr^{89} and Sr^{90} activity as indicators of the influence of latitude and altitude on the behavior of the stratospheric source.

Stewart (*3*) has provided data on Sr^{89} and Sr^{90} activity for rainfall collected at Milford Haven, Wales, over the period from April 1954 to April 1957. High-yield tests conducted during this period, together with the estimated strength of the stratospheric source in megatons of fission, are listed in Table 1. For the well-spaced Castle, fall 1955 Soviet, and Redwing tests, the Milford Haven data make it possible to distinguish behavior differences for stratospheric Sr^{90} sources of different location in space. When 170 is taken as the Sr^{89}/Sr^{90} production ratio and the midpoint of the test series is taken as the production date, the Sr^{89}/Sr^{90} activity ratios of Milford Haven rains for February to July 1956 identify about one-half of the Sr^{90} activity with the Soviet tests of the fall of 1955. Similarly, essentially all of the Sr^{90} for September to December 1956 is associated with Redwing test production. For the 18-month period following the Castle tests, the assumption that all Sr^{89} in Milford Haven rains was due to these tests can lead to only a small overestimation of the Castle contribution.

Figure 2 shows the Sr^{90} concentration of Milford Haven rains per megaton of stratospheric contamination following the Castle, Redwing, and 1955 Soviet tests. Each data point represents the Sr^{90} rainfall concentration divided by the stratospheric contamination in megatons (from Table 1) for the given test series. After each of the Redwing and 1955 Soviet tests, the data on Sr^{90} concentration were taken only for rains with a Sr^{89}/Sr^{90} ratio indicating a 50- to 100-percent Sr^{90} contribution from that test series. The results are very striking, showing an intensity of stratospheric Sr^{90} fallout soon after the 1955 Soviet tests which is 10 times that for Redwing and nearly 60 times that for Castle.

The large differences in north temperate latitude fallout intensities estimated for these tests can be attributed to several factors: (i) possible overestimation of the stratospheric component for equatorial tests; (ii) the degree of mixing of equatorial test debris into the Southern Hemisphere; (iii) differences in stratospheric storage times; and (iv) more selective deposition of Soviet test debris in north temperate latitudes. W. H. Langham (*6*) suggests that stratospheric injection of debris from surface-water shots may be only 30 percent. Use of this value would reduce the estimates of stratospheric contribution for Redwing and Castle given in Table 1 by factors of

Table 1. Estimated strength of stratospheric source for high-yield nuclear tests.* PPG, Pacific proving ground (includes Eniwetok and Bikini atolls).

Test series	Period	Location	Source strength (10^6 tons of fission)†
U.S. (Ivy)	Nov. 1952	PPG (11°N, 166°E)	1.4
U.S. (Castle)	Mar.–May 1954	PPG (11°N, 166°E)	20
U.S.S.R.	Aug.–Nov. 1955	(~ 52°N)	1.8
U.S. (Redwing)	May–Jul. 1956	PPG (11°N, 166°E)	6.7
U.S.S.R.	Aug.–Nov. 1956	(~ 52°N)	2.7
U.S.S.R.	Jan.–Apr. 1957	(~ 52°N)	2.7
U.K.	May–June 1957	Christmas Island (2°N, 157°W), Malden Atoll (4°S, 155°W)	
U.S.S.R.	Aug.–Dec. 1957	(~ 52°N)	5.3
U.K.	Nov. 1957	Christmas Island (2°N, 157°W)	
U.S.S.R.	Feb.–Mar. 1958	(~ 52°N), Novaya Zemlya‡ (75°N, 55°E)	3.3
U.S. (Hardtack I)	May–Jul. 1958	PPG (11°N, 166°E)	4.0
U.S.S.R.	Oct. 1958	Novaya Zemlya‡ (75°N, 55°E)	12.5–15
Total			~ 62

* U.K. tests of 3 high-yield devices, April and September 1958, not tabulated. See K. Telegadas (*6*). †The stratospheric component, based on stratospheric inventory estimates of Libby (*12*), assumes stratospheric injection of 20 percent for surface land shots, 80 percent for surface water shots, and 100 percent for air shots. ‡(*23*).

1.8 and 2.1, respectively. Assuming confinement of Soviet test debris to the Northern Hemisphere and equal partitioning of equatorial test debris between hemispheres, one could account for a factor of 2 of the difference in the values between Soviet and equatorial tests. Since most equatorial tests were conducted at 11° north latitude, however, a somewhat greater contribution to the Northern Hemisphere may be expected for them. Storage time and latitude selectivity factors, therefore, must account for most of the difference in the fallout intensities between Soviet and equatorial tests (Fig. 2). The factor of 6 difference between the values for Castle and for Redwing appears to be accounted for best on the basis of longer stratospheric storage times associated with greater cloud heights for the higher-yield Castle test shots.

The results are not inconsistent with mean stratospheric storage times of a few months or more for Soviet test debris in the polar stratosphere, 1 to 3 years for debris in the lower equatorial stratosphere, and 5 to 10 years for the debris at higher levels near the equator. In view of the marked influence of altitude and latitude on the storage time and the transport and distribution pattern of stratospheric fallout, the concept of a mean global stratospheric residence time is not applicable to the interpretation of stratospheric fallout.

Tungsten-185 Concentration

A substantial quantity of the W^{185} radioisotope was produced in the U.S. Hardtack tests during the period May through July 1958. Most of the W^{185} radioactivity was produced in several surface explosions of sufficient energy to thrust the debris into the lower stratosphere. A total production of 250 megacuries of W^{185}, corrected for decay to 1 August 1958, has been reported (*12*). An estimated 40 percent of the total was retained in the stratosphere, with most of the remainder deposited as local fallout. The stratospheric W^{185} component has afforded a unique opportunity to follow the fate of debris originating from the lower stratosphere near the equator for a single test series.

The concentration of W^{185} in individual rains collected at Burlington, Vermont, and Bedford, Massachusetts, is presented in Fig. 3. The W^{185} data are corrected for decay to 15 June 1958, an arbitrary date about midway through the production period. The estimated errors in W^{185} activity determination are shown, with large errors only for rain samples of low total W^{185} activity.

The W^{185} data exhibit a rapid rise over the May–July 1958 production period. The stratospheric origin of Hardtack Ba^{140} in New England rains during July to September 1958 (Fig. 1 and the related discussion) testifies to the concurrent stratospheric origin of the W^{185} activity. Except for a drop in December 1958 and an appreciable rise in April to June 1959, the mean W^{185} concentration, corrected for decay, remained approximately constant over the year that followed production. The scatter in the concentration data is due in part to the inverse relationship between rainfall intensity and isotope concentration, with the higher concentrations corresponding to the lighter rains (for examples, see *3* and *19*).

For several short periods, the data

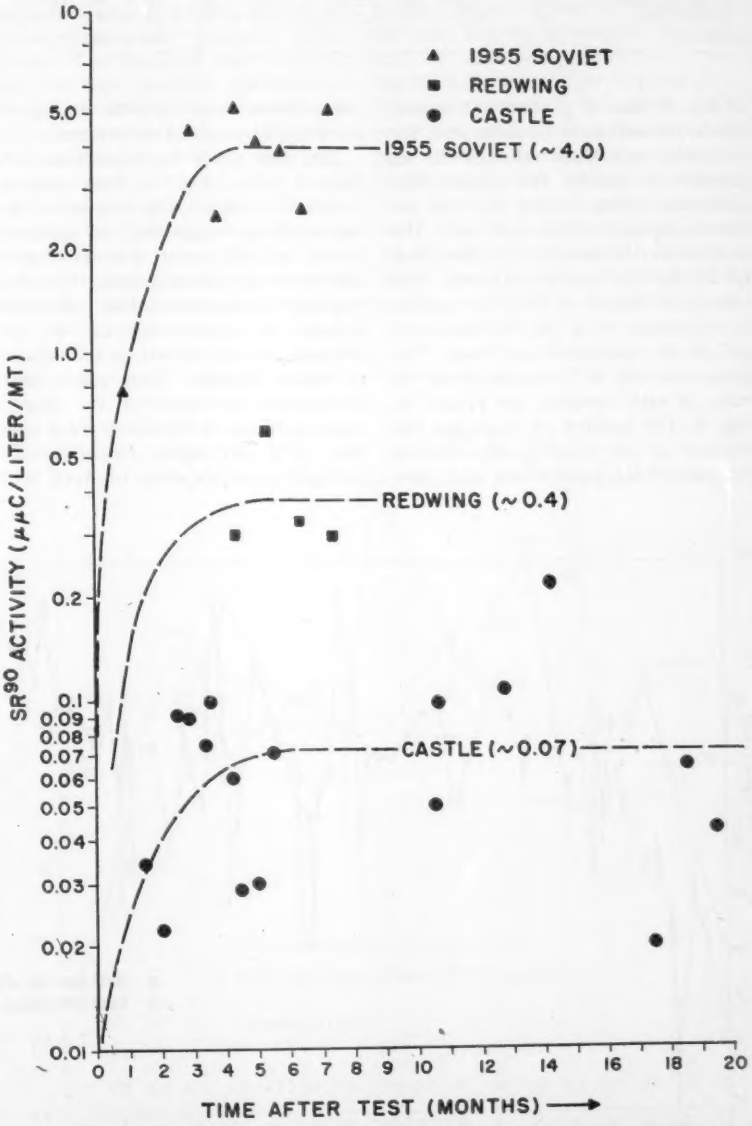

Fig. 2. Concentration of Sr^{90} in Milford Haven (Wales) rains per megaton (MT) of test source [from data of Stewart *et al.* (*3*)]. The source was identified by Sr^{89}/Sr^{90} ratio for well-spaced tests as follows: U.S. (Castle), March–May 1954 (11°N, 166°E); U.S.S.R., August–November 1955 (~ 52°N); and U.S. (Redwing), May–July 1956 (11°N, 166°E). The estimated strength of the stratospheric source, in megatons of fission, for each of these high-yield test series is given in Table 1.

Table 2. Rainfall data for Pôrto Alegre, Brazil.

Collection period	Rainfall (in.)	Specific activity on mean collection date (disintegration/min. per lit.)			Activity ratios corrected to 15 June 1958	
		Sr^{89}	Sr^{90}	W^{185}	Sr^{89}/Sr^{90}	W^{185}/Sr^{90}
1958:						
4 Jul.–1 Aug.	1.5	279 ± 19	4.63 ± 0.35	224 ± 19	91 ± 10	65 ± 8
1 Aug.–9 Aug.	4.4	242 ± 13	2.40 ± 0.34	191 ± 7	201 ± 30	131 ± 19
27 Aug.–5 Sept.	2.5	535 ± 240	5.30 ± 0.50	234 ± 8	274 ± 126	90 ± 9
6 Sept.–15 Sept.	4.6	94.4 ± 6.6	1.84 ± 0.20	80 ± 2	156 ± 22	97 ± 11
15 Sept.–30 Sept.	0.8	240 ± 21	2.45 ± 0.38	86 ± 14	350 ± 63	88 ± 20
25 Oct.–19 Nov.	1.4	162 ± 14	4.84 ± 0.72	105 ± 9	223 ± 39	84 ± 15
20 Nov.–2 Dec.	2.3	45.0 ± 3.5	2.73 ± 0.30	60 ± 6	138 ± 19	100 ± 15
2 Dec.–18 Dec.	4.0	33.5 ± 1.9	2.35 ± 0.19	41.9 ± 2.0	143 ± 14	94 ± 9
1959:						
22 Jan.–28 Jan.	1.7	14.9 ± 2.6	1.67 ± 0.11	19 ± 2	159 ± 30	87 ± 11
28 Jan.–30 Jan.	2.5	9.5 ± 2.2	0.96 ± 0.09	9.6 ± 1.9	187 ± 47	80 ± 17
30 Jan.–7 Feb.	1.7	19.0 ± 3.9	1.34 ± 0.16	25.5 ± 1.3	283 ± 67	159 ± 22
14 Feb.–25 Feb.	2.6	6.6 ± 1.9	0.72 ± 0.12	8.5 ± 0.8	236 ± 79	118 ± 23
26 Feb.–7 Mar.	0.7	19.2 ± 5.3	2.15 ± 0.22	12.0 ± 1.3	260 ± 77	62 ± 10
Average					208 ± 48	97 ± 17

of Fig. 3 show a gradual and almost regular transition in concentration for successive individual rainfalls. In an attempt to explain this observation, trajectories were plotted for the air masses associated with each rain. The method was identical to that described for the Ba^{140}/Sr^{90} activity-ratio data. The rains were classed as polar or tropical, in accordance with the indicated origin of the associated air mass. The mean monthly W^{185} concentrations of rains in each category are plotted in Fig. 4. The number of rains and the fraction of the monthly precipitation represented are noted beside each data point. Rains associated with air masses of doubtful origin were omitted.

The data on W^{185} concentration for tropical rains (Fig. 4) show only a moderate variation with time after initial build-up, suggesting an approximately uniform rate of downward mixing from the stratosphere into the tropical troposphere. The observed changes in concentration can be attributed, at least in part, to differences in rainfall intensity. Thus, lower concentrations are observed for heavy rains like those of September 1958 and July 1959 and higher concentrations for light rains like those of April and May 1959. The general decrease with time after initial build-up represents a gradual depletion of the stratospheric reservoir of W^{185}.

For rains of polar origin, however, the W^{185} concentration continues to build up until the spring of 1959 and then falls off more sharply in the summer of 1959. The slow build-up reflects the rate of mixing into the polar stratosphere as well as time variations in the rate of downward mixing at polar latitudes. The low W^{185} concentration of polar precipitation in December 1958 may be explained on the basis of reduced downward mixing due to a drop in height of the polar tropopause at this time. The very high concentration of W^{185} in polar rains in January, April, and June, 1959, may be explained partly by differences in rainfall intensity but may also be indicative of intensified downward mixing or subsidence of polar air. The large changes in relative W^{185} concentration in polar and tropical rains reinforce the conclusion drawn from the Ba^{140}/Sr^{90} data, that downward mixing is taking place from two regions of the stratosphere, separated in latitude. The remarkable reduction in W^{185} concentration in polar rainfall after June 1959 may be due either to virtually complete removal

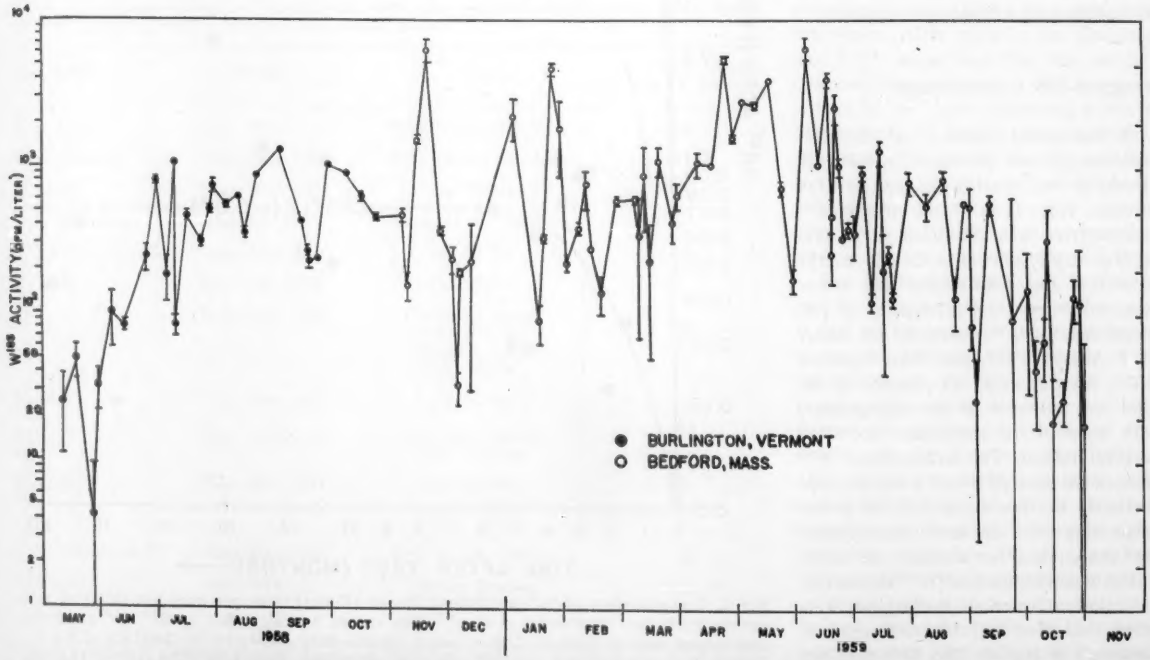

Fig. 3. Concentration of W^{185} in individual New England rains. Data are corrected for decay to 15 June 1958, assumed as the mean production date for the U.S. Hardtack tests.

of W^{185} activity from the polar stratosphere or to inhibition of downward mixing at this time.

It should be noted that the differences in W^{185} concentration observed for polar and tropical rains are not a direct measure of relative concentration in air. For a given concentration of particulate debris in air, higher corresponding concentrations in rainfall are to be expected in polar latitudes than in tropical latitudes, because polar rain clouds have a lower liquid-water content.

Origin of Recent Strontium-90 Fallout

Using W^{185} as a measure of Sr^{90} from the May to July 1958 Hardtack tests and Sr^{89}/Sr^{90} activity-ratio data for the period that followed the October 1958 Soviet tests to distinguish Sr^{90} fallout from that source, one should be able to distinguish the approximate contributions of Sr^{90} fallout from these two sources and from combined older sources. Data for radioactivity concentration in rainfall collections at Pôrto Alegre, Brazil, for an extended period following the Hardtack tests are presented in Table 2. The Sr^{89}/Sr^{90} data clearly indicate that most of the Sr^{90} fallout over the period of observation came from the 1958 U.S. and U.K. equatorial tests. The few large deviations in the Sr^{89}/Sr^{90} ratio from the average for the period must be due principally to the influence of tests conducted earlier or later than the assumed reference production date of 15 June 1958. The Sr^{90} data for closely spaced rains show the inverse relation between concentration of radioactivity and amount of rainfall. The gradual reduction in mean Sr^{90} concentration with time over the period of observation is attributed to the depletion of recent test debris from the lower layers of the stratosphere. In view of the evidence for an increase in stratospheric storage time with altitude for equatorial tests, the decrease in Sr^{90} concentration in Pôrto Alegre rains (Table 2) is not a reliable measure of the rate of depletion of the stratospheric reservoir.

The W^{185}/Sr^{90} data (Table 2) show a similar trend for Sr^{90} and Hardtack W^{185}. The observed W^{185}/Sr^{90} ratio of 97 ± 17 for the Southern Hemisphere, however, is substantially lower than would be expected from the reported Hardtack production data alone. Assuming that 40 percent of the reported total amount of W^{185} produced in the Hardtack tests and 0.4 megacurie of Sr^{90} from these tests was retained in the stratosphere, one obtains a value of 380 for the W^{185}/Sr^{90} ratio for the 15 June 1958 reference date. It is unlikely that uncertainties in production estimates account for much of the difference. Variations in W^{185}/Sr^{90} production ratios and in cloud heights for the individual Hardtack nuclear tests gave rise to initial differences in spatial distribution and may have resulted in uneven partitioning between hemispheres. The observed low W^{185}/Sr^{90} ratio for the Southern Hemisphere (Table 2) is attributed to the combined influence of these factors and to the Sr^{90} contribution of the 1958 U.K. equatorial tests.

The data on mean monthly Sr^{90} concentration in Bedford, Massachusetts, rainfall for 1958 and 1959 are presented in Fig. 5. These data are based on the Sr^{90} concentration of individual rains, weighted by precipitation amount. The two dashed curves connect points representing the estimated Sr^{90} contribution for the U.S. Hardtack tests and for the October 1958 Soviet tests, respectively. The Hardtack test contribution is estimated by assuming a W^{185}/Sr^{90} ratio of 380 for W^{185} activity corrected for decay to 15 June 1958. The October 1958 Soviet test component is based on the data on Sr^{89} activity. The total Sr^{90} activity was reduced by the estimated Hardtack Sr^{90} activity, based on the W^{185}-concentration data and the assumption that the W^{185}/Sr^{90} ratio was 380/170 in the Northern Hemisphere stratosphere at a reference date of 15 June 1958. This correction amounted to less than 10 percent except during June and August 1959, months for which corrections were about 10 and 20 percent, respectively. The Sr^{90} activity associated with the October 1958 Soviet tests was estimated from the corrected data on Sr^{89} activity, a Sr^{89}/Sr^{90} ratio of 170 at a

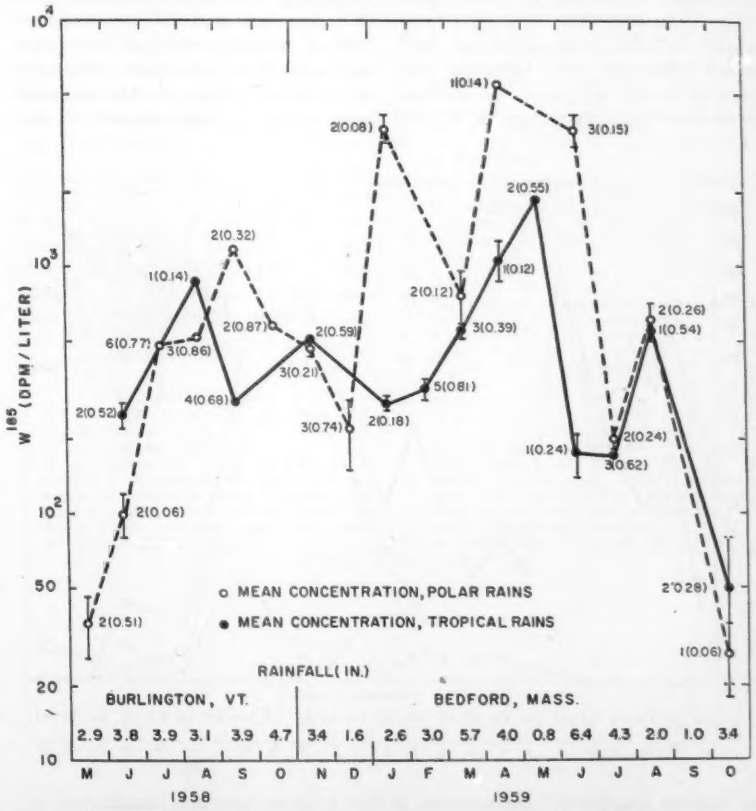

Fig. 4. Mean monthly concentration of W^{185} in New England rains classed by origin of air mass, from data in Fig. 3. Data are corrected for decay to 15 June 1958. Only individual rains for which air-mass trajectories indicated an unequivocal arctic or low-latitude source were used.

reference production date of 15 October 1958 being assumed.

It is clear that the heavy peak in Sr^{90} fallout over the first half of 1959 is due principally to the October 1958 Soviet tests. Immediately after these tests, the residual Sr^{89} activity from the Hardtack tests, as estimated from production amounts (Table 1) plus data on mixing between hemispheres and decay, accounted for less than one-thirtieth of the total Sr^{89} activity in the Northern Hemisphere stratosphere. For the first half of 1959, uncertainties due to errors in analysis in estimating the Sr^{89} component from the Soviet tests of October 1958 should not exceed 20 percent. A significant reduction in the assumed W^{185}/Sr^{90} ratio of 380 for Hardtack debris would account for more than the observed Sr^{90} concentrations. Similarly, it can be deduced from the Ba^{140}/Sr^{90} data (Fig. 1) and from Sr^{89}/Sr^{90} data that the heavy fallout during the first half of 1958 was due mainly to the Soviet tests of February–March 1958.

There is uncertainty about the Sr^{90} component from the Hardtack tests (Fig. 5) because of possible variations in the W^{185}/Sr^{90} ratio from the estimated value of 380. However, the curve accurately represents the relative mean monthly concentration of W^{185} in New England rains as determined from the individual-rainfall data of Fig. 3. The data show a marked increase in W^{185} concentration in Bedford rainfall nearly a full year after production. It is indicated (Fig. 4) that most of the increase is due to the much higher concentration in rains associated with arctic air. For New England, both the mean concentration in rainfall and the total deposition of W^{185} corrected for decay were nearly 2.5 times as high during the first six months of 1959 as they were during the last six months of 1958. By comparison, the concentration and deposition of debris from the Soviet tests of October 1958, on the basis of the analysis given in Fig. 5, was about 10 times as high in the first half of 1959 as in the latter half.

The dramatic removal in early 1959 of debris from these Soviet tests is attributed to intensified downward mixing or subsidence of air from the polar stratosphere in late winter. The spring peak for the decay-corrected concentration of W^{185} in rains associated with arctic air can be explained similarly. The very low concentration of both W^{185} and measured Sr^{90} in December 1958, a month in which all New England rains were associated with polar air, indicates a quite low rate of downward mixing at high latitudes at that time. The more rapid late-winter rise in rate of downward mixing of debris from the October 1958 tests as compared with the rate for W^{185} from the Hardtack tests may be due to differences in spatial distribution in the polar stratosphere.

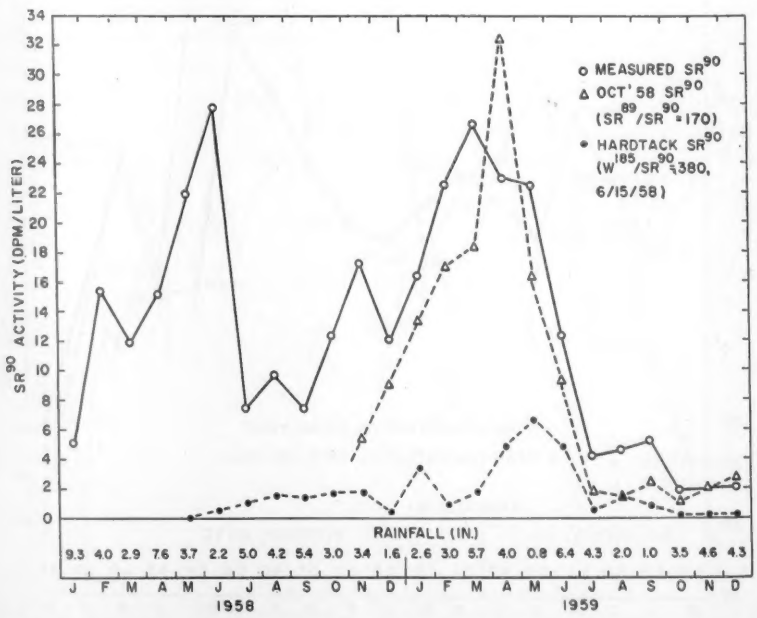

Fig. 5. Mean monthly Sr^{90} concentration in New England rains and contributions of the U.S. Hardtack (May–July 1958) and Soviet (October 1958) nuclear test series to Sr^{90} fallout. The Hardtack contribution of Sr^{90} is estimated by assuming $W^{185}/Sr^{90} = 380$ for W^{185} corrected for decay to 15 June 1958. The estimate of the Soviet October 1958 contribution is based on Sr^{89} data.

Summary

The foregoing discussion of fission-product ratios and concentrations of W^{185}, in which we have attempted to relate known sources of stratospheric debris to deposition in rains, has indicated a number of the complexities involved in the interpretation of atmospheric transport and deposition phenomena. The close spacing and variety of nuclear tests up to October 1958 and the fact that individual-radioisotope data of acceptable quality and type are limited have made it almost impossible to study the atmospheric behavior of individual nuclear-cloud sources. Even the W^{185} activity in equatorial latitudes was produced in a number of Hardtack tests of different date, yield, cloud height, and W^{185}/Sr^{90} ratio. Single, well-spaced nuclear explosions would offer a much-improved opportunity to study the physical aspects of fallout. Even in this case, however, the interpretation would be complicated by changes in the spatial distribution of the source with time under the influence of variable meteorological factors, and thus the results would have limited applicability at other seasons or in other years.

In spite of these complications and limitations, the isotope-ratio and W^{185} results have clarified a number of important points relative to the atmospheric transport of particulate debris from nuclear tests. The stratospheric origin of substantially all the short-lived, as well as the long-lived, radioisotopes in world-wide fallout appears to be well established for locations and periods for which reliable Ba^{140}/Sr^{90} and Sr^{89}/Sr^{90} activity-ratio data have been reported. The relative intensity of fallout per unit of strength of stratospheric source for different regions of the stratosphere shows a marked influence of latitude and altitude on fallout rates and fallout pattern. Debris injected into the lower regions of the polar stratosphere is substantially removed by subsidence or intensified mixing during the late winter and early spring. In this case, the storage time is a matter of months and depends on the time inter-

val between injection and removal. Storage times for debris in the equatorial stratosphere, however, are a matter of years and increase appreciably with altitude. Thus, the concepts of a well-mixed stratosphere and a mean stratospheric storage time appear largely inapplicable to the interpretation of stratospheric fallout.

Both the data on Ba^{140}/Sr^{90} ratio (Fig. 1) and the data on W^{185} concentration (Fig. 4) for rains classified in accordance with associated air-mass trajectory provide evidence for downward transport of debris from two regions of the stratosphere. The data on radioactivity indicate that downward mixing at high latitudes takes place to a limited extent throughout the year and is accompanied by a marked late-winter subsidence of the type described by Moser (*20*) and Dobson (*21*). Poleward movement of stratospheric air which must accompany this subsidence may account for the rapid appearance of early Hardtack debris in polar air (Fig. 1) and for the high W^{185} activity associated with polar air in the spring of 1959. The transfer of stratospheric debris into the temperate and tropical troposphere presumably takes place at middle latitudes near the subtropical jet. Downward mixing in this region continues throughout the year. The apparent seasonal variations in concentration of fallout in tropospheric air and rains at temperate and tropical latitudes to a large extent must be due to shifts in origin of the stratospheric air source and to changes in its concentration of radioactivity.

The sustained differences in isotope ratio and the large temporal changes in the relative concentrations of a given radioisotope in polar and in tropical rains indicate that tropospheric mixing of particulate debris across the polar front is inhibited. Thus, selective precipitation scavenging of radioactive fallout in frontal storms may be an important factor in explaining the selective deposition of fallout in north temperate latitudes. Seasonal distribution of rainfall, rainfall amount, average rainfall intensity, and the origin of the scavenged air are factors which markedly influence fallout accumulation. Areas receiving substantial rainfall in the spring, with a high proportion of light rains associated with arctic air, undoubtedly have had relatively high levels of fallout.

Thus, the marked differences in transport, distribution, and consequences for equatorial and high-latitude nuclear tests are becoming clearer. The rapid late-winter and early-spring removal of debris injected into the polar stratosphere leads to greater selectivity in the surface distribution. The corresponding short storage time makes the short-lived radioisotopes and their biological consequences more significant. The radioactive products of air bursts at high latitudes are distributed quantitatively in the form of widespread fallout within the hemisphere. Thus, the type, timing, and location of past Soviet tests have tended to maximize fallout in the heavily populated North Temperate Zone. The high proportion of surface explosions in the U.S. equatorial tests has resulted in deposition of the major fraction of the debris as local fallout. The resulting reduction in debris available for worldwide fallout, the more widespread distribution over both hemispheres, the longer residence in the stratosphere, and the relatively low seasonal and latitudinal selectivity of deposition have made fallout from equatorial tests substantially less significant per test unit than fallout from tests in other latitudes (*22*).

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Why Are Research Grant Applications Disapproved?

Characteristic shortcomings of rejected applications to the National Institutes of Health are described.

Ernest M. Allen

In the 12 months that ended 30 June 1959, the National Institutes of Health received and acted upon nearly 6000 competitive applications (*1*) for grant of funds to initiate or continue projects in medical and related biological research in the many research institutions of the country.

Of this number, approximately 2000 met with disapproval at the hands of the 30-odd advisory boards of scientists that give the applications technical review. These boards, known as study sections, are made up of distinguished scientists, active in research, who are connected with universities and other research institutions throughout the country.

That these "juries" of their peers are forced to render a verdict recommending disapproval of the grant applications of so large a number of scientists in a year's time is a matter of concern to those who contemplate it. Year after year the phenomenon continues to present itself, however, and even the most experienced scientific investigators may occasionally suffer.

We may liken the verdict of disapproval by a study section following its study of a research proposal to the decision by a group of medical consultants that a case before them is one of a certain acute illness. Illnesses have symptomatology and epidemiology. What is the epidemiology of the disorder we are considering, and what is its symptomatology? An epidemiological study is often a long-range and time-consuming undertaking, and is mentioned here only to emphasize a need. Symptomatology can, however, be described from a few collected cases, and it is the purpose of this article to indicate the symptomatology as displayed in a sample group of "disapprovals."

The sample consisted of 605 applications for grant of funds to initiate or continue research which were disapproved (*2*) in the round of study-section meetings in the spring of 1959. Thirty-three study sections were involved, representing the many different areas of research support in the over-all field of medical and related biological research covered in the National Institutes of Health's research grants program. The adverse comments made on these 605 research proposals prior to the vote on each are summarized in the minutes of the various study sections. These minutes have furnished the material for the analysis reported here.

As would be expected, a round-table discussion of a research proposal by from 15 to 20 scientists in the same general field, before vote on the question of approval, is not a mere voicing of stereotyped phrases. When a shortcoming of a research proposal is commented on, however, it is easy to decide whether it concerns the *problem*—the question the proposed research would seek to answer—or the *approach* by which the answer is to be sought, or the *competence*—the total of scientific judgment and technical skills—that it is proposed to mobilize toward the pursuit of the research. These three categories, with a fourth that has been called simply "other," have been made the basis for a primary classification of all shortcomings that were commented upon in the study-section minutes dealing with the 605 disapproved (*2*) research proposals. The results of this initial classification may be seen in the four italicized subcaptions in Table 1. It is seen that in 58 percent of the cases of the "disorder" we are considering, there are symptoms in the area of the problem: its importance or timeliness, in the general scientific area covered by the NIH program of research support, is not sufficient to warrant expenditure of NIH research funds on it. In 73 percent the approach is involved: the method of attack, as proposed, will not yield sufficiently useful data. In 55 percent the disorder manifests itself in inadequacy in the scientific competencies to be brought to bear upon the research, and in 16 percent there are other, miscellaneous manifestations.

In setting up these four classes it is recognized that failure to choose a meritorious or timely problem (class I) and failure to plan an adequate approach (class II) are reflections upon the investigator's judgment or the adequacy of his information regarding recent advances in the chosen area of research or in collateral areas. To combine class I and class II criticisms and put them in class III would, however, defeat the purpose of spreading out the spectrum of criticism to permit examination of the intensity of each portion.

In view of the fact that before a decision is made to undertake or continue a federal project of any kind, not excluding research projects, the proposal must be measured against the criterion, "Is this project necessary?," the absence of the specific criticism of lack of necessity may be surprising. If the specific criticism appeared at all, it would fall in class I. Clearly, no research can be regarded as necessary if it is "not likely to yield any new or useful information" (Table 1, criticism No. 1). The same may be said for the remaining adverse criticisms that make up class I. One or another of these was made in connection with 58 percent of the 605 applications. It can be said, therefore, that a verdict equivalent to "this research is not necessary" has a high rate of occurrence among the reasons for disapproval given in the study-section recommendations. If, on the other hand, the proposed research has *scientific merit*, if the approach is good, and if the investigator and his associates in the project can supply the necessary competence, then, in an era when scientific advance is clearly necessary, the project, promising as it does to contribute to such advance, must itself be deemed necessary. This is particularly true of basic research: needed "break-

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throughs" may come from it in almost any direction.

It should be emphasized that the criticism of lack of "competence," as the term is used in class III, does not necessarily mean that the investigator proposing the research is not a good scientist. The lack of adequate competencies could be simply a lack of sufficient acquaintance with the recent literature bearing upon the proposed research, or, say, failure to include provision for the collaboration of a well-trained biochemist in a proposed research study in which some advanced biochemical technique is called for.

The shortcomings pointed out by the study sections are given in Table 1, arranged under the four category headings. There are 26 entries in the list. Each of the 26 is a blend of closely related but variously phrased criticisms. The phraseology adopted in each instance can be regarded as a sort of verbal mode or mean for the group of criticisms it stands for. It is believed that the "averaging" has in no instance distorted significantly the meaning of any of the criticisms as actually phrased in the study-section records.

Just as the percentages corresponding to the four main classes add up to more than 100 percent, so also those within any one of the four groups add up to more than the percentage for that group. In both cases the excess is due to the fact that a given research proposal may have more than one adverse characteristic. For the 605 applications, one or another of the 26 criticisms occurred to an over-all total of 1558 times.

It may be noted that the criticisms are not mutually exclusive and, also, are not of the same order of either comprehensiveness or importance. Criticism No. 11, for example, is general enough to embrace Nos. 13 and 15. The criticisms far down the list in each of the first three classes are clearly of less importance than the leading items and in some instances would obviously not in themselves warrant disapproval of an application. Such differences are to be expected when the basic material for the analysis is, as here, taken from a summary record of free and unconstrained discussion of a research proposal.

The leading item in each of the first three categories is equivalent to "unqualified" or "not suitable." The problem is not qualified to be included among meritorious research problems (No. 1); or the approach is not suitable for this specific problem (No. 8); or the man is not qualified to conduct this specific research (No. 17). The second item (No. 9) under *approach* is comparable, except that it indicates that the applicant has failed to give enough information to permit the study section to arrive at a decision on whether or not the approach is suitable. Nebulous description of the problem (No. 7), on the other hand, is rare. Apparently the investigator finds it easier to define sharply the goal of his proposed research than to bring into sharp focus the route he proposes to follow. Most of the other items in each of the three large categories are equivalent to "*partially* unqualified."

Item No. 3, "the problem is more complex than the investigator appears to realize," might with good reason have been placed in class III instead of class I.

The criticisms that occurred in the discussions summarized in the study-section minutes were, of course, voiced primarily in an effort to arrive at a balanced judgment of the merits of each research proposal, not to aid investigators in perfecting their conception or description of research proposals. Some criticisms—for example, "the approach lacks scientific imagination" (No. 12)—would be of little help to the investigator if they were relayed to him. It is believed, nevertheless, that the total list of 26 adverse characteristics—the "symptomatology" for disapproved research projects—could well be

Table 1. Shortcomings found in study-section review of 605 disapproved research grant applications, April-May 1959. All percentages are to the base number 605.

No.	Shortcoming	%
	Class I: Problem (58 percent)	
1	The problem is of insufficient importance or is unlikely to produce any new or useful information.	33.1
2	The proposed research is based on a hypothesis that rests on insufficient evidence, is doubtful, or is unsound.	8.9
3	The problem is more complex than the investigator appears to realize.	8.1
4	The problem has only local significance, or is one of production or control, or otherwise fails to fall sufficiently clearly within the general field of health-related research.	4.8
5	The problem is scientifically premature and warrants, at most, only a pilot study.	3.1
6	The research as proposed is overly involved, with too many elements under simultaneous investigation.	3.0
7	The description of the nature of the research and of its significance leaves the proposal nebulous and diffuse and without clear research aim.	2.6
	Class II: Approach (73 percent)	
8	The proposed tests, or methods, or scientific procedures are unsuited to the stated objective.	34.7
9	The description of the approach is too nebulous, diffuse, and lacking in clarity to permit adequate evaluation.	28.8
10	The over-all design of the study has not been carefully thought out.	14.7
11	The statistical aspects of the approach have not been given sufficient consideration.	8.1
12	The approach lacks scientific imagination.	7.4
13	Controls are either inadequately conceived or inadequately described.	6.8
14	The material the investigator proposes to use is unsuited to the objectives of the study or is difficult to obtain.	3.8
15	The number of observations is unsuitable.	2.5
16	The equipment contemplated is outmoded or otherwise unsuitable.	1.0
	Class III: Man (55 percent)	
17	The investigator does not have adequate experience or training, or both, for *this* research.	32.6
18	The investigator appears to be unfamiliar with recent pertinent literature or methods, or both.	13.7
19	The investigator's previously published work in *this* field does not inspire confidence.	12.6
20	The investigator proposes to rely too heavily on insufficiently experienced associates.	5.0
21	The investigator is spreading himself too thin; he will be more productive if he concentrates on fewer projects.	3.8
22	The investigator needs more liaison with colleagues in this field or in collateral fields.	1.7
	Class IV: Other (16 percent)	
23	The requirements for equipment or personnel, or both, are unrealistic.	10.1
24	It appears that other responsibilities would prevent devotion of sufficient time and attention to this research.	3.0
25	The institutional setting is unfavorable.	2.3
26	Research grants to the investigator, now in force, are adequate in scope and amount to cover the proposed research.	1.5

used as a check list for criticism of grant applications by the investigator himself prior to their submission, no matter to what granting agency.

Summary

A list is given of 26 shortcomings mentioned repeatedly in study-section discussion of 605 research grant applications that were subsequently disapproved (*2*) (by vote, following the discussion). The shortcomings have to do with either (i) the conception of the research problems, or (ii) the proposed route of approach toward their solution, or (iii) the competencies to be mobilized toward prosecution of the research, or (iv) miscellaneous other matters. The percentage frequency of applications (in the total of 605) in which a given shortcoming was found is reported for each of the 26. Certain of the items in the list are discussed briefly.

Notes

1. Competitive applications are (i) applications requesting support for a new research project not previously supported by the NIH, and (ii) applications requesting *continuation* of support beyond the existing term of commitment. The "nearly 6000" applications referred to in the text consisted of 4600 proposals to initiate and 1200 to continue research; also, but not included in this analysis, there were 4500 awards of an additional year's support granted on study-section recommendations *previously* made and 650 (competitive) applications for additional funds to supplement awards already made.
2. Disapproval of an application by a study section has the force only of a recommendation to one of the National Advisory Councils. Council acceptance of the recommendation makes the disapproval official.

Charles Oberling, Research Worker on the Nature of Cancer

Charles Oberling, director of the Institute for Cancer Research Gustav Roussy of the University of Paris, was fortunate in his upbringing as scientist and man. It shaped him, as if purposely, to fight against the most challenging of diseases and to enjoy his life.

Oberling was born in 1895 in Metz, but his father—a postal clerk whom he ever remembered with admiring love—soon moved the family to Strasbourg. Here Charles was educated, gained entrance to the university, and studied medicine. But not without interruption. He joined the army in 1914, was seriously wounded twice, and only in 1920 became a doctor. Then fortune favored him again. The university had as professor of pathological anatomy the renowned cytologist Pierre Masson, inventor of revealing histological methods, who took Oberling on his staff. During eight years with Masson, Oberling received an intensive training in cytology and through this was enabled to discover new facts about the nephroses and the reticuloendothelial system, and to obtain a firsthand knowledge of tumor cells. He was assistant professor at Strasbourg when, in 1928, he was asked to become an associate professor of the Faculty of Medicine of Paris in a division dealing with histology, embryology, and pathological anatomy.

It happened that professor Gustav Roussy, who headed the division, was an experimentalist, ardently concerned with founding an institute for cancer research. Soon he had Oberling experimenting with him on the tumors of laboratory animals. In another two years he had achieved his institute, and Oberling was made its *chef de service*. Together they published a definitive atlas on the growth of the human central nervous system, but Oberling's work was now mostly experimental. He found out much about the transplantable growths of small mammals and the virus-induced tumors of fowls, and in addition he showed such ability as teacher and organizer that he was called to Strasbourg in 1937 to succeed Borrel in the chair of hygiene and microbiology.

Amédée Borrel is now a well-nigh legendary personage to French scientists concerned with the actuation of tumors. He it was who first ventured the view, in 1903, when little was sure about viruses, that they are the cause of tumors, and for this view he sought and fought throughout his later life. The times were hostile to it, for the hopes aroused in the Pasteurian epoch that tumors would prove to be due to microbes had been destroyed by tests of the newly transplanted growths of the rat and mouse: these growths yielded no other cause, on transfer, than their own living cells. Realizing through his own experimentation that this was so, Ehrlich had quit the tumor problem for others that he could solve.

Oberling's new academic duties required that he learn far more than he taught, and in strange fields. Hence, he did little on cancer before World War II began. By that time he had such a reputation as hygienist that late in 1939 he was asked by the Shah of Iran to reorganize medicine in Teheran and to try to make the city a more healthy place. Toward these ends he would be appointed dean of the Faculty of Medicine. He was working on war gases, but he was advised by the French Government to accept the offer. During two years in Iran he reformed the medical curriculum in the university, converted the city hospitals into university centers staffed with the best specialists, founded a school for nurses, and greatly improved sanitation throughout the immediate region. Then, with his task carried out, he journeyed in 1942 to the United States to undertake cancer research anew, but upon landing was again asked to come to Iran, this time to improve health throughout the country.

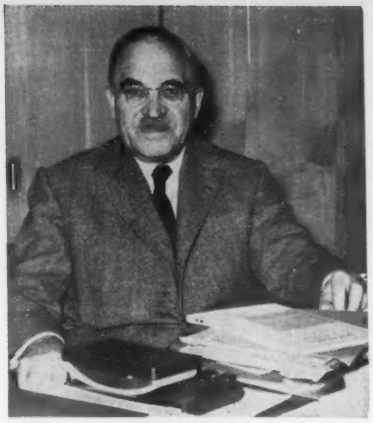
On the way back he had the first of the cardiac "strokes" which were to harass him throughout his remaining 17 years. Feeling unequal now to the task in Iran and unwilling to return to an occupied France, he brought his wife and boys to the United States and tried to enter its army. He was rejected because of his medical history, and so he became pathologist-in-chief at the Mary Imogene Bassett Hospital in Cooperstown, New York. The directors of that enlightened institution could not have known that in appointing him they acted to further cancer research even

to this day. For during his stay at Cooperstown he not only carried out his hospital tasks with engaging gusto, lecturing on war pathology in addition, but, stimulated by his environment and aided by an excellent library, he completed his book, *Le Problème du Cancer*. It was published in 1942 and was admirably translated as *The Riddle of Cancer* by William H. Woglom, previously editor of *Cancer Research*, as was also a revision in 1952. The book has since gone into several editions and has appeared in Spanish and German. It tells what is known about cancer with that limpid clarity one likes to think of as French; although written with zest for the lay scientist, it is backed by references, and specialists continue to gain from it because of the author's enterprising thought.

Oberling was still dean in Teheran University, and now the Fighting French urged him to return to Iran. He did so in 1944, ranging the whole country, inspecting local conditions and hospitals, starting medical schools in Meshed, Shiraz, Isfahan, and Tabriz, and radically bettering the situation.

In 1947 Roussy died and Oberling became his successor; for although he had done almost no laboratory research during the previous ten years, his book had been potent in stimulating it. Long before, while he was still with Roussy, he and M. Guerin had discovered that a virus producing and maintaining a transplantable leukemia in fowls would cause solid growths as well. Thus they were led to perceive not only that the leukemia studied was a neoplastic disease of blood cells but that a single virus can cause various tumors. Thus the problem presented by their diversity was simplified. On the basis of these facts, and others set forth in his book, Oberling had become convinced, even before succeeding Borrel, that viruses cause tumors in general. Since then several neoplastic viruses of unprecedented subtlety and scope had been discovered in mammals. Furthermore, the electron microscope had opened a new era in cytology, and it had actually revealed *in situ* the particles of some tumor-producing viruses. At last one could study the minute pathology of tumor cells, not only as such, but with special reference to whether they owe their state to the action of viruses. To Oberling these were the most important of tasks.

A curious situation had developed concerning the chicken tumors. Many



Charles Oberling

such growths had been induced by means of chemical agents, but from none had a causative virus been obtained, whereas every "spontaneous" fowl tumor adequately tested had yielded one. Furthermore, the spontaneous tumors propagated for years had undergone a morphological simplification (like many mammalian neoplasms of unknown cause when thus maintained). In both these respects they differed from the recent, chemically induced growths. Hence the old supposition was brought forward anew that they could not be true neoplasms. Nature had to accord with artifice, else she could not be real. But Oberling, working once more with Guerin, induced in 1950 a fowl sarcoma with 20-methylcholanthrene that not only had the morphological characters of a "spontaneous" tumor but yielded a causative virus. Other workers have recently obtained a causative virus from rat lymphomas induced by radiation.

As time went on, repeated cardiac episodes wore Oberling down; yet he could still achieve through his able and devoted young associates. Together they proved with the electron microscope that the virus particles causing fowl tumors have a form so characteristic as to be readily distinguishable from normal cell constituents. Following the intracellular cycle of these viruses electronically, they found that it takes place entirely in the cytoplasm, as does the cycle of the milk virus responsible for the nodules from which mammary mouse cancers arise. Also they saw particles characteristic of the fowl viruses scattered in certain organs of healthy chickens. Oberling came to realize that some viruses pass through a phase in which electron microscopy fails to disclose them, and he asked himself whether in many tumors they may not persist only in this form yet be responsible for the neoplastic state. His group were engaged in comparing the ultrastructure of human tumor cells with that of normal cells of the same sort throughout his last years.

Oberling served not only as director of research but as a teacher and lecturer also in the Faculty of Medicine of Paris, and in 1949 the university appointed him professor of carcinology, creating this post, with its apt title, especially for him. In 1956 he was made professor of experimental medicine in the *Collège de France*, a chair that was once Claude Bernard's. Both distinctions meant more duties, and these came while his physical powers were being lopped away. Nevertheless he labored on and even journeyed to far-off spots on occasion. To see him at the end of a grueling symposium was to feel compassion, so exhausted was he in all save mind. Yet when invited, shortly before his death, to join a gathering of virologists in New York and give an address that would render it memorable to one of their number, a personal friend, he came, and concealing how weary he was, spoke even better than had been hoped. A few days after returning to France in January 1960, he fell ill, and a few weeks later an emergency operation disclosed an unsuspected, widely distributed cancer. He died within a few hours. Had he known of his disease he might have remarked, as a spur to research, that man still walks in the shadow of cancer all his days, just as throughout his long past, though now so often rescued.

Charles Oberling was a naturalist in a widely diverse sense of the term. He delighted in phenomena, and his life had many facets. As a boy in Strasbourg he fell under the spell of the arts and was strongly inclined toward music. He had indeed the artistic temperament, yet with it an urgent sense of responsibility. The gift of admiring was his; he always saw things to enjoy. The interlude in Iran was to him a resplendent adventure because he soon came to love the people and their country. Fair-minded and humorous, comprehending and wise, his presence, wherever he went, gave personal happiness to workers against disease.

PEYTON ROUS

Rockefeller Institute, New York, New York

Science in the News

Regulating the Drug Industry: Reports Ask for Reforms While the Industry Leaders Ask for Trademark Protection

Among the outgrowths of Senator Kefauver's drug investigation was the appointment, by Arthur Flemming, Secretary of the Department of Health, Education, and Welfare, of two committees to look into charges of lax performance by the Food and Drug Administration. One was a group of educators and scientists organized under the National Academy of Sciences, which was to look into the scientific side of the question; the other was a triumvirate of government officials with investigative experience who were to look into charges that decisions of FDA officials had been unduly influenced by their cordial relations with the industry they were charged with regulating.

For both committees the key case was that of Henry Welch, the now-retired chief of FDA's antibiotics division, who had earned a quarter of a million dollars in half a dozen years as a result of his interest in a number of scientific journals supported by advertising from firms marketing antibiotics. Welch's superiors at FDA knew of his association with the journals. He was openly the editor. They did not know the "honorarium" he said he was receiving for this service was a share of the advertising and reprint revenues, or that one of the reasons the publications showed no profits was that Welch's honorarium was running to $40,000 a year.

The National Academy committee was asked to see whether the decisions of FDA officials, particularly those in the antibiotics division headed by Welch, were scientifically sound. The committee was also invited to make any general recommendations it cared to make about the working of FDA.

The second committee, working out of the office of HEW Secretary Flemming, was to make sure, first, that there were no more Welches working at FDA, and second, whether the general relations of FDA employees with the drug industry were not cordial to the point where FDA was inclined to be unduly generous in seeing things from the industry point of view.

The National Academy committee filed its report in October. It found the decisions in the sample of FDA cases it had studied "acceptable," but went on to make 11 recommendations for enlarging the powers or revising the procedures for regulating the drug industry. The recommendations in general paralleled the recommendations made by FDA itself last summer (*Science*, 17 June) under the pressure of the Kefauver investigation, and Flemming endorsed all of the committee's proposals except one, regarding advertising, whose effects extended to other agencies of the government beyond his control. Flemming promised to study the feasibility of the proposal on advertising. The tone of the National Academy report was that FDA is doing a decent enough job considering the limitations of budget, of personnel, and of legal powers under which it must work, but that all of these will have to be enlarged if FDA is going to do a really satisfactory job.

Personnel Investigation

The second committee, investigating personnel, has yet to file its report, but it has leaked enough of its feelings to foreshadow its findings. These are that, although there are no more such blatant conflict of interest situations in FDA as Welch represented, the relations between FDA and the drug industry are indeed much too cordial. The committee would like to see a return to the arm's length stance of FDA that prevailed from the time of the New Deal through until about the end of World War II. Unlike the recommendations of the National Academy committee, this does not find much sympathy in FDA, where the official view is that FDA can now accomplish a good deal that is useful by enlisting the cooperation of industry, something which would be difficult to do if the position of FDA were changed from regulator to policeman.

Any major policy changes at FDA will have to be confirmed by the new administration, but everyone assumes that the FDA under the Kennedy administration will go at least as far in asking for legislative reforms as the FDA did at its testimony before the Kefauver committee last June. It is assumed that the new HEW secretary will reaffirm Flemming's endorsement of the proposals of the National Academy committee.

Whether the new secretary will move to carry out the recommendations of the personnel investigating committee over the objections of career officers at FDA is less clear. Much will depend on how convincingly the investigators can document their case. No one seriously supposes that Democratic and Republican leaders in Congress are any less determined in fighting for their points of view on controversial matters because their cordial relations allow them to work out informal arrangements for cooperating on numerous subsidiary matters. The same applies to attorneys on opposite sides of a law case and even to Soviet and American diplomats across an international conference table. Everyone, except the most naive, recognizes that such cordial relations not only make life pleasanter for the antagonists, but very substantially expedite the business at hand.

The problem at FDA, as at other government regulatory agencies, is that cordial relations are not on quite as reciprocal a basis as they are among attorneys and legislators and diplomats. The industry people have generous expense accounts with which to entertain the government personnel, and jobs to offer them in the event they should choose to leave government service. There is the danger not of cordiality between the regulators and the regulated, which is useful, but of the regulators' coming to forget that, despite the room for a great deal of useful cooperation, the regulators and regulated do, or should, after all, represent opposing interests and opposing points of view.

A good case can be made, though, that the effectiveness of an agency in safeguarding the public interest is more closely related to the tone set by the

Administration and top political appointees in an agency than by the personal relations of the career civil servants with their contacts from the industry.

A recent survey of FDA employees showed that half of them had considered leaving the agency in the past year. The problem of getting and keeping capable employees is especially severe with scientific personnel who in government have neither the salaries that go with employment in private industry nor the amenities that go with an academic career. Hedging them round with regulations implying they are not to be trusted does not help the matter, but neither, on the other side, does a feeling that their bosses, the Administration, or the relevant Congressional committees are interested in cooperating with industry, or in cutting the budget to the point where the staff feels that either budget paring or seeking cooperation has taken precedence over seeking to protect the public interest.

In at least one area the FDA cannot be accused of leaning over too far to see the industry's point of view. Several of the major companies have lately been encouraging publicity for the counterfeiting drug problem, partly in hopes of pressuring the FDA into expending more of its resources in combatting the problem.

"An insidious racket that threatens the health of every man, woman, and child is spreading throughout America," announced *Parade*, a Sunday newspaper supplement. "The racket," said *Parade*, "is a flourishing under-the-counter trade in fake and diluted drugs, stamped with the counterfeit trademark of reputable firms." In fact, so far as FDA officials can tell, the racket does not necessarily, or even normally, involve either fake or diluted drugs. It stems from the situation of which the Kefauver committee made so much: that a great many drugs are sold at a modest price under their generic names (e.g., reserpine) and at a much higher price under their trade names (e.g. Serapsil, the trade name under which Ciba sells reserpine). The economics of the drug industry which account for this situation are reasonable or unreasonable, depending on the analyst's point of view, but the mere existence of the situation places a great temptation before the retail druggist: the temptation to substitute unbranded drugs for all or part of a prescription specifying a specific brand. The druggist feels the patient is no worse off, having received the right medicine, while the druggist is a good deal better off, having pocketed the difference between the price of the branded and unbranded drug. (If the prescription had merely called for the drug by its generic name the customer might have gotten the unbranded drug at its proper price.)

Substitution is illegal, and there is a fair chance of the druggist's being caught unless the substituted pills happen to appear to be indistinguishable from the specified brand. Here the temptation extends to the small drug firms, most of which do not actually manufacture drugs, but simply buy the chemicals in bulk form and manufacture pills. A number of firms make a specialty of making pills that look like those of the higher priced brands. This is unethical but, in most states, prefectly legal, and the availability of such goods further increases the temptation of the druggist to substitute by reducing the likelihood of his being caught.

In the game's fully developed form, the risk and a share of the extra profits are passed from the retail druggist to the distributor, who assures the druggist that his pills not merely look like the high priced brand, but really are the high priced brand, obtained at a bargain price and therefore for sale at a bargain price. In the slim chance that the druggist is caught, probably by a detective employed by the large drug firm to make purchases and send the prescriptions to the plant for analysis, the druggist can stoutly claim that he has done nothing wrong, but must have been taken in by a deceitful wholesaler. One major drug firm says it found that 12 percent of all prescriptions written for its products were partially or wholly filled with unbranded substitutes.

An Old Problem

The problem has existed for years, but it is only in the past few months that the industry has begun to seek publicity. Until recently the major firms preferred to hush up the situation. They feared that talking about the problem would merely cause the public and the medical profession to lose faith in the extra assurance of first quality in a brand name. There would be no point in paying a premium for a well-known brand if in fact there was a good chance you would actually get not merely unbranded drugs, which are usually, but not always, as good as the well-known brand, but unbranded drugs handled and distributed by people whose ethics are highly questionable.

The new attitude of those firms that have been seeking publicity for the situation stems partly from the feeling that the problem has grown to the point where it is getting to be prohibitively expensive for the brand owners to finance a private policing system themselves. They would like to arouse the public to demand stricter enforcement by state and federal authorities and stiffer penalties for proven offenders, who now tend to get off with very light sentences.

Public Health

The companies make the point that the racket not only costs the legitimate manufacturers a good deal of money, but that the public health is being endangered since a man who is counterfeiting trademarks can hardly be trusted to make drugs at all. But the problem, from the public's point of view, involves more than cracking down on firms for counterfeiting trademarks. The same man with the same ethics and the same manufacturing procedures who is counterfeiting may also be selling legitimate unbranded drugs of inferior quality. The National Academy report endorsed the Food and Drug Administration's proposed legislation for strengthening its powers to regulate and supervise the manufacture of all drugs, a step which would help keep inferior drugs from reaching the public whether masquerading as well-known brands or not.

The curious aspect of this effort to arouse the public to a special phase of the problem of regulating commerce in drugs is that the sensationalist articles, illustrated by suitably horrendous photographs of the interior of a raided firm, arranged for by publicists for a drug company, may well do more good in the long run than the eminently sensible report of the National Academy committee. For the scientists' report, after all, only restates the sort of recommendations that knowledgeable people have been making for years. The publicity about drug counterfeiting may result in considerable pressure on legislators to do something, while the National Academy report is being read mainly by people who are already convinced that something ought to be done.—H.M.

News Notes

New Children's Science Book List Issued by AAAS Library Program

The AAAS Science Library Program has begun distribution of its new *Science Book List for Children* to all state school systems in the United States. The list, which contains 1105 titles, is designed as a guide to recreational and collateral reading in the sciences, including mathematics, for elementary-school students up to and including the eighth grade, and as an acquisition guide for elementary-school and public libraries. Its publication by the AAAS was made possible under a grant from the National Science Foundation.

The Selection Process

The new list is based substantially on selections by four college students outstandingly competent in science. In establishing the selection process, the AAAS followed the counsel of the late eminent historian of science George Sarton, who said: "Such elementary books deserve to be criticized with particular care, but it is very difficult to find reviewers who are willing, competent, and reasonable. Good scholars are often too snobbish and supercilious to judge elementary books as they ought to be judged, severely with regard to essentials, leniently with regard to details, kindly always. Perhaps the best judge of an elementary book is a young man, not yet too far removed from the elements, provided he is sufficiently modest and generous."

The four undergraduates chosen to assist in evaluating the scientific content of the books were William B. Blacklow of Ohio Wesleyan University; Delio Gianturco of Georgetown University; Robert W. Lynn of Haverford College; and Douglas E. Miller of the University of Michigan.

The selections were reviewed by an advisory group of librarians recommended by the American Association of School Librarians, who appraised the books for potential interest, suitability of content, and vocabulary and designated comparative difficulty. Finally, the list was reviewed by Hilary Deason, director of the AAAS Library Program, in collaboration with Ruth N. Foy, library consultant for the Baldwin-Whitehall Schools, Pittsburgh, Pa.

Deason comments that the list gives "preference to books that are the most valuable for the purpose of developing a child's mind and teaching the basic concepts of science. No fiction has been included. We have excluded whenever possible those books that are overly sentimental, that are anthropomorphic in style, and that cover so much subject matter superficially that the reader actually learns nothing by reading the book. Such books convey erroneous ideas and are repugnant to many bright children.

"The best science books for children are those that deal with a single basic idea, concept, or subject, which is developed accurately and completely, using whenever possible the appropriate technical terms. By reading such books a child may progress from the very simple book to those that are a little more difficult in accordance with his natural interests and curiosity, and thus each book read will add to his total fund of knowledge."

Developed specifically as a guide to the purchase of books (other than textbooks for class use) under provisions of Title III of the National Defense Education Act, the list is recommended by the AAAS for adoption by state educational departments and local school systems as a standard which satisfies the requirements of the Act. Any interested person may obtain a copy of the list by sending $1 to AAAS headquarters in Washington.

Other Science Library Programs

Distribution of the *Science Book List for Children* is only one of the Science Library Program's activities supported by the NSF. The Traveling High School Science Library Program, initiated in 1955, will in the 1960–61 school year lend a collection of 200 science books to more than 1600 high schools in every state and territory. The Traveling Elementary School Science Library, begun in 1959, will for the same period lend a collection of 160 science books to 800 elementary schools and central libraries.

The popular annotated list of paperbound science books, entitled *An Inexpensive Science Library*, goes into its fourth edition this year. It gives recommended reading for high-school and college students and for the general adult public. The book is available to schools on request and to the public at 25 cents a copy.

European Science Institute Proposed at NATO Conference

Senator Henry M. Jackson (D–Wash.) last week urged establishment of an International Institute of Science and Technology in Western Europe, an "M.I.T. of Western Europe," to help assure "the continuing scientific preeminence of the NATO Community." Jackson, chairman of the Scientific and Technical Committee of the NATO Parliamentarians Conference, meeting in Paris, made his proposal in his science report to the conference.

Jackson pointed out that educational institutions like the Massachusetts Institute of Technology have played a central role in promoting scientific and technical development in North America and that Western Europe has no comparable institutions. Expressing doubt that any one European country, by itself, could mobilize the financial and manpower resources needed to establish the proposed science center, Jackson added that "the Western European nations acting *in concert* could easily mobilize the money, skills, and facilities required."

In support of his plan, Jackson also criticized the view that NATO is only a military alliance: "NATO was formed and exists to promote cooperation in every field where the 15 members of our Community can do together what they cannot do apart. What better way to re-affirm this cardinal principle of our Community than by applying it to the problem of education in science and technology."

Oceanographic Cruise Ends

The research vessel *Chain* of the Woods Hole Oceanographic Institution returned to her home port in mid-November after a 5-month cruise which took her north of the Arctic Circle and into five European seaports. The expedition had a strongly international aspect, with scientists from several European nations aboard. In addition, during various portions of the trip the American scientists worked with the Norwegian research vessels *Helland-Hansen* and *H. U. Sverdrup*, the British *Discovery II*, and the Scottish *Explorer*.

Most of the work was in the North Sea, the Norwegian Sea, and the North Atlantic between Iceland and the British Isles. A variety of old and new

techniques was used to examine the ocean waters, the bottom sediments, and the underlying crust. Many seismic observations were made at locations suggested by the foreign scientists in connection with their own research projects.

Almost continuous use was made of the thermistor chain, a 600-foot instrument-carrying chain that is towed astern to get continuous temperature recordings from the upper layers of the water. It was towed nearly 20,000 miles.

About three dozen scientists took part in the expedition for varying lengths of time. Among them were ten students doing summer work at the oceanographic institution.

Planet Earth Film Series Completed

The National Academy of Sciences has announced the completion of its "Planet Earth" film series. Produced under a grant from the Ford Foundation, the series includes 13 16-mm, 27-minute educational films, available in both color and black-and-white, covering the principal fields of geophysical research which have been stressed in connection with the International Geophysical Year.

The film series, the academy's first, synthesizes man's knowledge of his physical environment and also delineates the powerful new tools for gathering data on space and the cosmos, such as rockets and satellites. Extensive footage was shot for the series in all parts of the world, both during and after the International Geophysical Year, to provide the viewer with a stimulating film of field work being done in his own country and in distant places.

Although the inspiration for the program came from the IGY, the films give a rounded picture of man's quest for knowledge in each field, outlining the principal discoveries and ideas and raising questions, concerning both the cosmos and the earth itself, that still challenge science. While utilizing the striking results of the IGY, the films range in content from early ideas and experiments to current ones, with some projection into the future.

In the production of the films, specialists in each of the 13 fields, from this country and abroad, were called upon for guidance. Because many of the ideas in geophysics are abstract, animation is used as needed.

Hugh Odishaw of the National Academy of Sciences is director of the series. Lothar Wolff of Louis de Rochemont Associates, Inc., is the producer. The series was produced by the academy in cooperation with the WGBH Educational Foundation, Cambridge, Mass.

To assist in the development of the films, an Advisory Committee on Education (IGY) was established by the president of the academy. In addition, a working group drawn from the U.S. Office of Education, the National Science Teachers Association, the National Education Association, the National Academy of Sciences, and the National Science Foundation gave advice on production of the series from the standpoint of stimulating interest in science in general and geophysics in particular.

The academy has concluded an agreement with the McGraw-Hill Book Company, Inc., under which McGraw-Hill will distribute the films, either severally or in sets, both in the United States and abroad, to educational and research institutions at a price of $80 for the black-and-white film and $150 for the color film. The entire series is now available for preview at McGraw-Hill Text-Films, 330 W. 2nd St., New York, N.Y.

U.S. Sponsors Space Research at Northern Ireland University

Queen's University in Belfast, Northern Ireland, may become one of Great Britain's chief centers of pure research in space as the result of a recent agreement with the U.S. Advanced Research Projects Agency. The agreement contracts for the largest amount of financial assistance ever provided by the United States Government in this field in Western Europe. Cost for the first year is estimated at about $182,000, and support will continue at the rate of about $56,000 a year for approximately 5 to 7 years.

The project will be conducted by the university's applied mathematics department, headed by David R. Bates, who suggested the space program when he visited Washington last year. Bates's department is engaged in two main activities—upper-atmosphere space research and study of the properties of atoms and molecules. It is the latter field which is being expanded. Although the research will have obvious military implications, it will not be secret.

One immediate result of the new agreement is an arrangement for scientists from Belfast to spend periods of from 3 to 4 months in U.S. research plants. Another is the establishment at Queen's University of a digital computing unit. Alexander Dalgarno is director. Money is also to be supplied for additional research staff for the unit.

News Briefs

Basic research institute. The Basic Health Research Institute, a nonprofit entity chartered by the State of Illinois, has recently been transferred to Tucson, Ariz., and has started operations in a small building there. Current and planned programs are concerned with cellular processes, considered from various angles, such as growth, fertilization, behavior, and physiology.

Additions to the group of researchers will be made only after careful study of the qualifications of those concerned. Each member must find his own financial support, although sponsorship of the institute will be available when applications are to be filed with grantor agencies. Overhead from grants is enabling the institute to supply necessary facilities and services until it is sufficiently well established to enlist the support of private foundations. For further information, communicate with Dr. Beatrice Gelber, President, Basic Health Research Institute, 509 North Santa Rita Ave., Tucson, Ariz.

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Biology film program launched. Roman Vishniac, a leading photographer of microscopic life, will produce the first part of a series of educational films entitled "Living Biology," under an initial National Science Foundation grant of $112,340 awarded recently to Yeshiva University. The 18-month grant will cover production of eight 28-minute films for use by secondary schools and five 45-minute films for colleges and universities. All will be 16-mm sound-color pictures.

These 13 films are only the first part of a larger program. The complete series will include 40 films, 24 for high-school use and 16 for college use. The work will take 3½ years to complete.

The Audio-Visual Center of Yeshiva University, headed by Sidney Pleskin, and the university's film library will promote and distribute the "Living Biology" series under direction of the National Science Foundation.

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Yale science center. Yale University has received a gift of approximately $10 million from C. Mahlon Kline of Philadelphia for the construction of a new science center. Kline, a graduate of the Yale Sheffield Scientific School, is honorary chairman of Smith, Kline and French Laboratories. The new center, which will be known as the Kline Science Center, will consist of a chemistry laboratory, a library and laboratories for the biological sciences, a geology building, and a central auditorium for the sciences.

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AEC reactor training. Thirty-seven scientists and engineers—15 from eight foreign countries and 22 from the United States—have started reactor technology training at the Atomic Energy Commission's Oak Ridge National Laboratory. The group is enrolled in the third session of 1-year courses in either nuclear reactor hazards evaluation or nuclear reactor operations supervision. The two courses were started in early 1959, following announcement of the programs in 1958 at the Second United Nations International Conference on the Peaceful Uses of Atomic Energy at Geneva. Students from noncommunist foreign nations and the United States may enroll.

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Canadian Medical Research Council. The Canadian National Research Council has announced the establishment of a Medical Research Council with responsibility for all activities formerly conducted by NRC's Division of Medical Research. The new council will have virtually complete autonomy but will function under the general administration of the National Research Council.

The setting up of the Medical Research Council is an interim measure pending the government's future consideration of appropriate legislation. The eventual establishment of a completely independent Medical Research Council was implied in a statement in Parliament last summer. Chairman of the new body is R. F. Farquharson, professor emeritus of medicine, University of Toronto and vice-president (medical) of NRC.

Grants, Fellowships, and Awards

Atomic energy. Students in chemistry, engineering, mathematics, or physics may pursue graduate studies under Atomic Energy Commission special fellowships in nuclear science and engineering which are administered by the Oak Ridge Institute of Nuclear Studies. Some 150 fellowships are available, for first-, intermediate-, and terminal-year graduate study at a school selected by the fellow from a list of more than 50 designated universities. Stipends range from $1800 to $2200 per year. An additional $500 is allowed for a spouse, and $500 each for a maximum of two dependent children. Tuition, fees, and a limited travel allowance are also provided.

Graduate students, or seniors who will have received their degrees by the beginning of the 1961–62 academic year, may apply for a fellowship to begin in the fall of 1961. Applications must be received by *6 January 1961*. For further information, write to the Nuclear Science and Engineering Fellowship Office, Oak Ridge Institute of Nuclear Studies, Box 117, Oak Ridge, Tenn.

Fertility. The Lalor Foundation has announced its 1961 program of awards in support of research on the fundamental biochemical and physiological mechanisms concerned with fertility and the early stages of reproduction in various forms of life. The awards may range up to $8000 per year, depending upon the scope and duration of the projects approved. Preference will be given to younger members of university and college faculty and staff, with an upper age limit of 41 years. The work may be carried on at the applicant's own institution or elsewhere.

The foundation will also grant postdoctoral summer or short-term research awards at the Marine Biological Laboratory, Woods Hole, Mass., or elsewhere, for appropriate projects in the fields specified. These awards will normally not exceed $1000 for a single man or a woman, $1200 for a married man working at his home institution, and $1350 for a married man with his principal program at another institution.

Requests for information and for application forms should be directed to the Lalor Foundation, 4400 Lancaster Pike, Wilmington 5, Del. The final date for receipt of executed application forms, complete with supporting data, is *16 January 1961*. Notification of appointment will be on or before 15 March.

Psychometrics. The Educational Testing Service is offering for 1961–62 two fellowships in psychometrics leading to the Ph.D. degree at Princeton University. These are renewable research fellowships of $3750 per year, plus dependency allowances, which provide for part-time training in the general area of psychological measurement in the Princeton offices of the Educational Testing Service, in addition to the normal program of graduate studies at Princeton University. To be considered, a candidate must either have taken the Graduate Record Examinations in 1960 or register by 6 January to take these examinations on 21 January 1961. Fellowship applications must be submitted before *6 January 1961* to the Director, Psychometric Fellowship Program, Educational Testing Service, Princeton, N.J.

Scientists in the News

The Royal Society has announced the election of two new foreign members, both of the United States. They are **George W. Beadle**, chairman of the division of biology at California Institute of Technology and former president of the AAAS, and **George B. Kistiakowsky**, professor of chemistry at Harvard University and chairman of the President's Science Advisory Committee.

The National Science Foundation, which administers the United States Antarctic Research Program, has announced the appointment of four scientists to top posts at U.S. antarctic stations. In addition, two men have been named to represent the United States in programs operated cooperatively with Argentine and Australian scientists. The new appointees, listed below, will arrive in Antarctica this month and will remain from 1 year to 15 months.

George H. Meyer of the University of Texas will be station scientific leader at McMurdo, where geology, biology, and exploration geophysics projects are being carried on by a nine-member staff. Meyer will continue the bacteriological survey of the McMurdo Sound area that he initiated a year ago.

Norman S. Benes, who was meteorologist-in-charge at Hallett Station in

1958, will be scientific leader at Byrd Station, where he will coordinate the activities of 11 scientists working in meteorology, geomagnetism, seismology, ionospheric physics, auroral physics, and glaciology.

Ben W. Harlin, a U.S. Weather Bureau meteorologist of Louisville, Ky., will direct scientific work at the South Pole Station for the coming year. He participated in the International Geophysical Year-Antarctic program as meteorologist-in-charge at Little America Station, 1957–58.

Robert W. Titus of Reno, Nev., and Santa Rosa, Calif., also a U.S. Weather Bureau meteorologist, will head the scientific program at Hallett Station, where the U.S. is engaged in cooperative research with New Zealand.

Two other Weather Bureau meteorologists have been assigned to cooperative scientific programs. **L. David Drury** of St. Louis, Mo., will be the U.S. representative at the Ellsworth Station on the Weddell Sea Coast, where the U.S. and Argentina conduct research. **John E. Breckinridge** of Binghamton, N.Y., will be the U.S. senior representative at Wilkes Station, where U.S. and Australian scientists are conducting research.

Vincent D. Perry, vice president and chief geologist of the Anaconda Company, will receive the 1961 Jackling Award of the Society of Mining Engineers, a constituent of the American Institute of Mining, Metallurgical and Petroleum Engineers, on 1 March 1961 during the annual meeting of AIME, to be held from 26 February to 2 March in St. Louis. Perry, who is being honored for his contributions to geology and geophysics, will deliver the annual Jackling lecture after the presentation.

A. F. Frederickson, formerly of the Pan American Petroleum Corporation, has been appointed chairman of the department of geology at the University of Pittsburgh. Before he joined Pan American, Frederickson was a professor of geology at Washington University (St. Louis).

A. C. T. North, member of the Medical Research Council's staff at the Davy Faraday Research Laboratory, The Royal Institution, London, is spending the academic year at the Massachusetts Institute of Technology, in the department of biology.

Richard S. Caldecott, geneticist with the U.S. Agricultural Research Service and associate professor at the University of Minnesota, has taken a 2-year leave of absence to work as a geneticist with the Division of Biology and Medicine, U.S. Atomic Energy Commission, Washington, D.C.

Sidney L. Pressey, professor emeritus of psychology at Ohio State University, has been given an honorary degree by that institution for his pioneering work on educational automation, acceleration, and the life-span approach to problems of psychological development.

Frank H. Healey has been promoted to research and development director for Lever Brothers Company, in which post he will be responsible for operations at the company's Research and Development Center, Edgewater, N.J. Healey previously was development manager for processing.

James V. Warren, well-known cardiologist and chairman of the department of internal medicine at the University of Texas, will become chairman of Ohio State University's department of medicine on 1 April. He will succeed the late **Bruce K. Wiseman**, noted hematologist, who died in March.

Martin J. Swetnick, chief physical scientist of the Defense Atomic Support Agency's Radiation Division since 1958, has resigned to accept a position with the National Aeronautics and Space Administration, where he will be responsible for the instrumentation of the lunar and planetary exploration programs.

Stephen J. Smith, physicist in the atomic physics section of the National Bureau of Standards, has been appointed chief of the section. Typical work now underway in the section includes determining the properties of negative ions from photodetachment and electron collision studies, and developing a rubidium vapor frequency standard.

Herbert McKennis, Jr., professor of pharmacology at the Medical College of Virginia, who has recently returned from 6 months as visiting professor in the University of Chile's Institute of Physiology, has been made an honorary member of the Chilean university's faculty of medicine. He has also been named an honorary member of the Society of Biology of Santiago.

Paul E. Gagnon of Canada has been appointed director of the International Atomic Energy Agency's Division of Exchange and Training of Scientists and Experts. Gagnon, a chemist, has been associated with Laval University, Quebec, since 1921. In 1940 he was appointed governor of the university and also dean of the graduate school, positions which he held until his transfer to IAEA.

Recent Deaths

Bert S. Butler, Tucson, Ariz.; 83; former professor and head of the University of Arizona's department of geology and mineralogy in the College of Mines; 13 Nov.

John B. Gibson, Upton, N.Y.; 33; associate physicist at the Brookhaven National Laboratory; 15 Nov.

John T. Manter, Augusta, Ga.; 50; assistant professor of neurology and microanatomy at the University of Georgia College of Medicine; 5 Nov.

William E. Mordoff, Ithaca, N.Y.; 70; professor emeritus of engineering at Cornell University; served Cornell 42 years before retiring in 1956; 15 Nov.

Joseph W. Roe, Bridgeport, Conn.; 89; professor and chairman of the department of industrial engineering at New York University from 1921 to 1937; past president of the Society of Industrial Engineers; 9 Nov.

Emery A. Rovenstine, New York, N.Y.; 65; internationally known anesthesiologist and professor and chairman of the department of anesthesiology at the New York University Medical Center; 9 Nov.

Margaret L. Varley, Chestnut Hill, Mass.; an instructor in public health education at Boston College and at the Simmons College School of Nursing; assistant professor at the Harvard School of Public Health, 1950–57; in 1947, went to Egypt, Iraq, Iran, Syria, and Lebanon to establish nursing schools for the Rockefeller Foundation; 8 Nov.

Leonard Worley, Manhasset, N.Y.; 55; professor of biology at Brooklyn College and deputy chairman of the department; specialist in histology and cytology.

Robert E. Wright, Canberra, Australia; 33; research microbiologist at the Division of Plant Industry, CSIRO, Canberra; specialist in the genetics of respiratory factors in yeast; 3 Nov.

Book Reviews

Tropical Africa. vol. 1, *Land and Livelihood.* George H. T. Kimble. Twentieth Century Fund, New York, 1960. 603 pp. Illus.

During its present session, the United Nations General Assembly added 16 former colonies in sub-Saharan Africa to its membership. This brought to 21 the total of self-governing, independent states in tropical Africa which are now members of that body. (The grand total of sub-Saharan members is 22, but I follow Kimble here in omitting the Union of South Africa from the "tropical" African community.) While some of these, notably the former Belgian Congo, will for a time undoubtedly experience difficulties in constructing nations or national communities out of their ethnically and linguistically heterogeneous components, there is no doubt that the combined political and economic potential of the infant African states is already forcing the creation of new patterns of diplomacy on the international scene. For even if the birth and early growth of these African states is accompanied by internal dissension, the rest of the world, on both sides of the Iron Curtain, knows that the countries themselves will last, that they will "westernize," and that, singly or collectively, they will assume a growing importance in the West's economic and political struggles for survival.

Even countries like Kenya, the Federation of Rhodesia and Nyasaland, Angola, and Mozambique, which have not yet severed their colonial ties or freed themselves from dominance by an immigrant European minority, will play an increasingly important role in the international competitions of the present decade. Indeed, the inevitable efforts of their African populations to rid themselves of European control and to join the growing roster of independent African states will be an important factor in determining the nature of this competition. Some experts have even speculated hopefully that, because the stakes are so high and the alternatives so alarming, the efforts of the African people to extend rule by the majority to every corner of the continent south of the Sahara may throw the competition more fully into the controlled arena of the United Nations. In short, they believe that international peace and prosperity may have a far better chance of survival *precisely because* the Africans are willing to fight for the right to rule themselves and to develop their economies in line with the interest of their indigenous populations rather than in the interest of a metropolitan power or an immigrant white settler minority.

Whether these experts are right or whether those who take a more pessimistic and tinderbox view of the Africans' efforts to achieve independence and self-rule are right remains to be seen. But whatever the ultimate consequences of the stunning series of events that have taken place in sub-Saharan Africa during the last few years, one of the immediate effects has been to force an awareness of the former "dark continent" on an overwhelmingly provincial American public. And, although maps are soon outdated and the place names change with confusing rapidity as country after country achieves independence, the general continental outlines are far better understood today than they were ten—or even four or five—years ago. Unquestionably, the curiosity for more accurate information is also growing.

It was with an apparent realization of this growing interest in tropical Africa and of the need for a compendium of basic data against which to project the changing scene that the Twentieth Century Fund persuaded George H. T. Kimble, the distinguished geographer, to prepare a modern handbook on that part of Africa that lies roughly between the Sahara on the north and the Limpopo River on the south, between the tropics of Cancer and Capricorn. Of sub-Saharan Africa only the Union of South Africa is not included in this two-volume survey, toward the preparation of which 46 experts contributed "working papers." "More than a thousand" others were interviewed by Kimble in the course of his research, which spanned several years and three continents.

Land and Livelihood consists of 13 chapters, beginning with an introduction to the "economic life" of the area at the turn of the century. This is followed by a detailed chapter on the physical features of tropical Africa, its climate, vegetation, and soils; a chapter on "racial constitution," the distribution of the people and other demographic characteristics. The next two chapters are concerned with European and African agriculture and with European and African settlement patterns and resettlement schemes. There are lengthy and detailed chapters on the forests—their composition, present use, and potential—and on the waters and their use for power and as a source of food (present and potential); there is a chapter on the distribution of mineral wealth and its potential; and there are chapters on industrial progress, transportation, markets and marketing, and the labor force.

The expository materials are supplemented by 49 tables dealing with a variety of subjects, from rainfall distribution to the sex ratio among the adult population and the balance of payments in Ruanda-Urundi. Thirty-four maps portray graphically many things, from air flow to manioc production and the international migration of Africans between 1946 and 1952.

While the materials included are neither as complete nor as dispassionately outlined as those of Hailey's *African Survey* (Oxford University Press, 1957) nor, for all of their readability, as racy as Gunther's *Inside Africa* (Harper, 1955), they are quasi-encyclopedic in scope. More than 40 "different managements" are discussed; there are more than 600 groups who "do enough things differently from their neighbors to be recognizably different" (page 5); more than 167 million people are "dealt with" in this compendium.

In his acknowledgements, Kimble tells us that "No book is entirely the author's own, and probably few books are less so than this one. It might even be contended that the main reason for having a single name on the title page is that somebody had

to take responsibility for the mistakes!" Certainly no book that tried to encompass such a vast area as tropical Africa in all of its variety, that attempted to deal with all aspects of culture and natural environment as well as some of the main features of recent history, could have been produced unaided by a single individual. It is well that Kimble sought the assistance of 46 experts. It is well, too, that he made himself liable for any mistakes. For a book of this size is bound to have them, and it would be ungentlemanly to foist these on the contributors when the author himself has so obviously reworked the materials into a unitary and highly readable account.

In a foreword, August Heckscher reminds us that Kimble is not only a scholar, but the kind of scholar who "does not assume that scholarship precludes good writing." The writing is very good indeed. In respect to style, this first volume of *Tropical Africa* may be one of those answers to the prayers of social-scientists—and especially of non-social-scientists—who wish publications in this field were more often readable. But readability is not enough. Nor is exhaustive research and the aid of a corps of experts. The product of such a massive collective effort may be evaluated only by the accuracy of its results and the insights of its analyses. In these respects, Kimble's first volume has, as might be expected, certain virtues and certain deficiencies.

For the reader eager to acquaint himself with some of the complexities of a "continent in turmoil," Kimble has organized and presented, in most palatable form, a generous collection of data about a staggering number of things. Much of it will still be useful after the last colonial power has transferred political control to the resident African majority. In this regard the volume may be recommended as one among several reference works to be consulted by those who are anxious to acquaint themselves, in the most general way, with certain recurrent or idiosyncratic features of tropical Africa's natural habitat and culture.

But while the author remarks several times on the internal diversity of the area under scrutiny, his organization and approach lead frequently to overgeneralization, sometimes to a facile "homogenization." I suppose these are flaws which are almost inevitable in the construction of a handbook of this kind. In fact, only the enormous quotient of ignorance about Africa and the urgent need for dispelling some of that ignorance with facts presented in a readable form at all justify the publication of so ambitious a volume.

The book will not please the scholarly Africanist-at-large; even less will it please the institutional and regional specialist on Africa. It appears to have been aimed primarily at an audience whose knowledge of Africa is either slight or nonexistent. Perhaps it is for this very reason that the African "expert" might feel especially disturbed by some of the uses to which Kimble puts his facts; by the way he sometimes proceeds to judgments, analyses, or conclusions with insufficient data; by evaluations made on the basis of data taken from only one side of a controversial issue—what one might call the built-in bias of selectivity; or by the gratuitous value judgments and personal prejudices he occasionally offers in place of analysis.

Let me cite some examples of the kind of thing I am referring to:

1) What does the author mean when he suggests that some of the post-emergency Kikuyu are "thinking better . . ." page 142)?

2) How would he defend the imputation of African infantilism in the following? "Many . . . Africans like nothing better than to buy more of the things they already have" (page 392).

3) What does he mean when he observes that the task of fashioning strong economies in the new territories is pressing and "calls for gifts of mind as well as of "matter" and then adds, "It is indeed fortunate that so many of these territories have the latter. The former may well prove the harder to come by" (page 369).

4) On the basis of what kind of data or research does he render the following observation on the generalized African "mind" as related to diet? Africans like "to eat and drink the things a European eats and drinks. A man is what he eats and drinks. . . . Accordingly, if the European is a smarter man than the African—if he can make stronger 'magic,' can earn more money and have more leisure in which to enjoy it—it must be because of what he eats and drinks." And that is why "canned goods, packaged cereals, refrigerated meat, butter . . . beers . . . wines and spirits" are in such rising demand among Africans (page 514).

5) What kind of data lead Kimble, when referring to one of the new resettlement schemes for Europeans and Africans in Mozambique, to the conclusion that the inferior bush-housing of the Africans in their part of the community owes to "diverse social and linguistic backgrounds . . . but there is no thought of segregation" (page 180 ff.)? On page 309 of *Portuguese Africa* (Harvard University Press, 1959), James Duffy suggests that the Africans' isolation and inferior circumstances in this settlement area are altogether involuntary.

6) On instincts: "The trading instinct is strong . . . in many Africans" (page 567).

7) On the African's capacity to learn and the European's capacity to earn: "The training of a man to do even the simplest of new things, such as pushing a wheelbarrow, frequently takes an amount of time out of all proportion to the gains to be derived by the employer" (page 586). Many employers must be willing to pay the price, for I saw hundreds of wheelbarrows being used in East Africa, and not one of them was being pushed by a European.

8) This one will probably come as a great surprise to physical anthropologists. "Here and there [in Africa] the spade of the archaeologist has unearthed the skeleton of a man a million years old . . ." (page 22).

9) And what of the author's definition of the Federation of Rhodesia and Nyasaland as a "self-governing, multiracial state based on equal rights for all civilized men and opportunities for all men to become civilized" (page 538)? Wouldn't this look like an oversimplification to some of the hundreds of Africans who were beaten, jailed, and otherwise mistreated during last year's demonstrations on behalf of "equal rights?"

10) And what of the analysis involved in the author's conclusion that pilfering by African "storekeepers, ticket takers, and inspectors" owes to impaired "ethical standards" which should be elevated (page 411)? Could it be possible that it is not ethical standards alone which need elevation, but African wages as well, if the pilfering is to diminish? It's worth a try; and at the very least it is an amendment which might have been proposed by the author himself.

11) In the course of a factual and informative discussion of the tariff union that links Kenya, Tanganyika, and Uganda, Kimble points out that the union does not "affect in any way the right of each territory to vary its

own tariff." Export duties have thus come to differ considerably among the three territories. The author's analysis: "The reason for this is probably less fiscal than psychological, it being colonial nature to disagree with neighbors" (page 544).

12) In the concluding chapter of volume 1, "The workers," Kimble tackles, among others, the difficult job of explaining the slow rate of African advancement to better jobs. He concludes that the problems begin with government or trade union intervention (pages 592–93). "So long as these assumptions [about which job belongs to which race] remain unwritten and uncodified [many employers will be] happy to have an African demonstrate his competence since it means that they can cut labor costs by replacing high-priced with lower-priced help. The trouble starts when these assumptions get written into the laws of governments and trade unions. Then African advancement of an evolutionary kind becomes much more difficult, for the division of the labor field between European and non-European is clearly defined, carefully guarded by the European workers' watchdogs, and not readily amenable to 'boundary' adjustment." Many Africans, I believe, would question whether the informal and desultory decisions of certain employers counted for evolution at all. But even if employer self-interest were to insure an increase in the number of this kind of boss at a fairly rapid rate, that would not solve the problem of the African who is looking for *equal* pay for equal work. Government intervention or the action of trade unions removes the element of whimsicality from the contractual relationship and offers the hope of protection to the African, not only in his competition with European workers but in his efforts to secure just treatment from all employers.

Despite the shortcomings which are illustrated in some of the foregoing excerpts, and despite the tendency to frequent oversimplification, there is, I believe, much information of value in this volume, and all of it is presented in a charming prose style. If it falters from time to time, it is not only because the subject is too large, or too diverse, or too complicated, but because the author occasionally tries to make it seem smaller, more homogeneous, and less complex than it is.

The future of the new Africa, as its precolonial past, rests largely with the African himself. For, as Kimble reminds us "The tropical Africa of today is the work of the African's hands; almost every railroad, highway, public and private building, mine, plantation and European farm is a monument to his physical exertions" (page 575). The tropical Africa of tomorrow will even more surely be a monument to the *whole* African. For in it, we may hope, he shall be freed, as never before during the period of his colonial status, to contribute not only the strength and skill of his hands but the creativity of his intellect to the making of new nations and the growth of a continent.

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Tropical Africa. vol. 2, *Society and Polity*. George H. T. Kimble. Twentieth Century Fund, New York, 1960. 506 pp. Illus. Set, $15.

In the early 1950's when the Twentieth Century Fund turned its attention to Africa, it was not as obvious as it is today that rapid political and economic developments would soon place Africa in the forefront of the world scene. At that time all of tropical Africa, with the exceptions of Liberia and Ethiopia, was in colonial status, and most people, even the most informed, expected it to remain so for many years to come. Planning for economic, social, and political development was the concern of the colonial powers. When the United States dealt officially with tropical Africa, its dealings were largely with the appropriate officials in the metropoles. Few African students reached this country. Most Americans thought of Africa, if at all, as a country of big game and safaris, a field for mission work, or a romantic land of darkness. They knew nothing of the new industrial developments, of the efforts to further social and economic change, of the growth of towns and urban problems, of the appearance and hopes of African trade unions, of the growing group of young African leaders who were demanding a share in the governing of their own countries and were soon to refuse to accept anything save fully independent African governments. Only a few American scientists and scholars had actually worked in Africa and knew something of its problems at first hand, and these tended to write for their fellow specialists.

Today tropical Africa consists of a score of independent nations and a few colonial territories which will undoubtedly obtain independence in the next few years. African political leaders are frequent visitors to the United States where they expect to find support for their programs and an understanding of their problems. The United States is increasingly involved in technical assistance in the newly independent countries. The number of American scholars and scientists with a specialized interest in Africa is proliferating rapidly. Universities and colleges are teaching an increasing number of courses on Africa, and books about Africa pour from the presses.

All this makes apparent an urgent need for a convenient general handbook, incorporating the mass of specialized knowledge now available on various portions of the continent, to serve the student in the classroom, to supplement the knowledge of the specialist, and to provide a guide for the general reader. The Twentieth Century Fund is to be congratulated for the vision with which it foresaw this need and for the impressive body of expert knowledge it was able to mobilize for the project. Some 46 reports on special topics were prepared as background material from which the final report, *Tropical Africa*, was assembled by George Kimble. The Fund was wise in placing this last task in the hands of a geographer, for of all specialists it is the geographer who most clearly holds to a widened vision when he writes of the way in which man and his environment interact. As a popular handbook, the work also gains by being the final product of one writer, for it has a coherence derived from a single style and a particular point of view that a collection of essays, no matter how well assembled to complement one another, cannot have. At the same time, most readers will wish that it had been possible to publish the background papers as a supplement to the handbook. Their value to the scholar becomes apparent on almost every page of the two volumes. It is also to be wished that the Fund had found it possible to publish the annotated bibliography prepared for it by the International African Institute.

Tropical Africa will find its place on the bookshelves of the specialist who will turn to it as one convenient means of checking conditions in portions of

Africa where he has not worked and for background information in fields which impinge upon his own. It will also become, no doubt, the standard guide for the nonspecialist who only wants to know about Africa. In many ways it is a very good guide, though it is not the only one in its field. At the time it was planned the only general survey of African conditions was the old edition of Lord Hailey's *African Survey*, then almost 20 years old and sadly out of date. This was completely rewritten and reissued in 1957 and remains the most authoritative source on Africa, though its style is not one to attract the general reader. No doubt the scholar will prefer its succinct pages packed with detailed information to the more lively prose of *Tropical Africa* where the flow of analogies and metaphors obscures the facts and, on occasion, seems to crowd them out all together. Nevertheless, even the reissue of *The African Survey* is now some four years old, and for more recent information *Tropical Africa* is now the most convenient source.

It is also probable that the scholar will prefer volume 1 of *Tropical Africa*, for there the chapters deal very largely with material things which can be counted, measured, and placed on maps, where it is possible to give evaluations which rest on more or less accepted standards. The treatment therefore tends to be factual. The general reader no doubt will prefer volume 2, for it deals with economic, political, and social developments which are more likely to interest him and which he will feel to be more immediately pertinent as background to the events with which he is concerned. Recent political developments have been more fully described in other recent books, but here they find their setting amidst the background of poverty, illiteracy, inadequate medical facilities, difficulties caused by the terrain, and the scarcity of competent technologists with an understanding of tropical conditions. The reader can therefore assess not only constitutional developments and the programs of political leaders, but also the problems with which these leaders must cope if their peoples' demand for a better life is to be met.

It is also a strength of *Tropical Africa* that the viewpoint is not solely that of the towns and of the young educated leaders who dominate so much of the current political scene. These are certainly important, but Africa is still largely rural. Many of its people are conservatives with no great liking of, or respect for, many of the changes urged upon them. In this world of the countryside, much of the urge for change has come from the outside, often through European administrators and technicians who have striven to introduce new methods and institutions to people uncertain that they are desirable or worth the effort required of them. Something of the problems involved in successfully bringing about technical or institutional changes is made clear in the chapter on community development. With the emergence of the independent nations, the impetus to change given by the old administrators and technical officers will vanish. At the same time the development funds made available during the past 20 years by the various colonial powers can be expected to diminish. It will now be for the new African elite, whose origins and ambitions are sketched in another chapter, to channel the ambitions of their people behind schemes for economic and social change and, at the same time, to find funds to finance the projects. It is too soon to say whether the magic of independence will prove more fruitful than the old compulsions in assisting Africa to overcome its poverty and to provide a basis for rapid advance.

Volume 2 has a fascinating story to tell, and much of it, especially the long quotations from the background papers, is extremely valuable. Unfortunately it cannot be recommended as one would wish. A tendency to editorialize pervades the volume, and implicit and explicit biases distort the presentation. The first two chapters, on indigenous patterns of social life and on social change, are particularly bad in this respect. They will make the anthropologist cringe and wonder if 30 years of research can have been for naught. They will infuriate any Africans who read them, and rightly so. It is utter nonsense to write that Africa was in the Stone Age when the Europeans arrived. Iron-working was a well-established craft throughout Africa at the time and had been for a thousand years and more. African trade in iron, copper, and other metals was carried on for centuries before the Europeans came and stopped it. It is equally absurd to write that Africa had no centralized governments before colonial rule was established. If by this is meant no centralized governments on the present scale or on the present models, well and good; otherwise a most casual knowledge of African conditions in the 19th century should have prevented such a statement. Both statements seem to be derived from a desire to stress the lag between European and African development. The same bias appears in the remark that "Africans like children" select some things and reject others. This is reminiscent of the old racialist myth that dark people and children are somehow alike and irrational, while Europeans are adult and rational. This is 1960, and such statements have long since disappeared from serious scholarly writing.

These are merely three instances, though flagrant ones, of the bias which distorts the first two chapters of this volume and which appears in somewhat less obvious form in the more factual chapters that follow. In a curious fashion, the volume seems pervaded by the thought and feelings characteristic of the 1940's. The implicit assumptions remain those of the colonial era, rather than those of the new period into which Africa is now moving. The tone is paternalistic and moralistic. There is a strong Christian bias, which colors the account of the role of Islam in Africa. If one can ignore all this, then there is much to appreciate in the chapters which describe administrative and political structures in the various African countries, methods of financing development, educational systems and the work of adult education, methods of community development, the work of the various churches, the organization of African trade unions, the control of disease, and the role of the new elite.

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X-Ray Absorption and Emission in Analytical Chemistry. H. A. Liebhafsky, H. G. Pfeiffer, E. H. Winslow, P. D. Zemany. Wiley, New York, 1960. 367 pp. Illus. $13.50.

This book is intended for the analytical chemist who is entering the field of x-ray spectrochemical analysis, and thus far it is the most comprehensive book published for this purpose. The authors have covered almost every subject of interest to the intended reader, some more completely than others, but this is to be expected when dealing with a subject which overlaps several scientific fields.

The first chapter deals with the theory of x-ray generation and the properties of x-rays; this is followed by a chapter on x-ray detectors. The succeeding chapters deal with specialized fields of interest, including absorptiometry, film thickness determination, and x-ray emission spectrography. There is a chapter containing excellent descriptions of presently available equipment, and one on the elementary statistics of x-ray measurements. The final chapter deals with special topics which are closely related to the general field—for example, gamma-ray absorption and emission, x-ray point sources, and applications in the biological sciences.

The book is very clearly written and readable, and it will provide the analytical chemist with a considerable amount of the information necessary for entering the field. Especially useful are the discussions of sample preparation and sample handling.

The book's value is further enhanced by the tables of wavelengths and constants given in the appendixes as well as by a bibliography of element determinations.

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Physical Methods of Organic Chemistry. vol. 1, part 1 of *Technique of Organic Chemistry*. Arnold Weissberger, Ed. Interscience, New York, ed. 3, 1960. xii + 918 pp. $24.50.

The expansion of volume 1 of this excellent series on the technique of organic chemistry reflects both the volume's popularity and the editor's awareness of the need to include additional chapters on physical methods. The general excellence of format and printing has been retained, but I believe that fewer monographs should be included in each volume. The cost is high, but it is commensurate with the value of the monographs.

Part 1 has gained four new chapters and has lost five by transfer to Part 2. The new chapters are "Automatic control" by J. M. Sturtevant; "Automatic recording" by D. R. Simonsen; "Weighing" by A. Corwin; and "Determination of particle size and molecular weight" by G. B. Beyer. The first is a very concise introduction to the general principles of automatic control and includes most of the essential parts of the chapter on temperature control (by J. M. Sturtevant) which was included in the previous edition. The chapter on automatic recording presents a brief introduction to the general characteristics of various types of recorders. These two chapters, although they contain a judicious selection of material, are too brief to be of great practical value to the majority of chemists. The excellent chapter on weighing fills an evident gap in the earlier editions. The author has packed into 57 pages a great amount of valuable information on the design and testing of balances and on the procedures of weighing. Beyer's chapter on particle size and molecular weight fills in admirable fashion the need for a concise and systematic account of the methods used to characterize systems having broad distributions of particle size, as well as the need for a résumé of the type of results most characteristic of these methods.

Chapters retained from part 1 of the second edition are: "Density" by N. Bauer and S. F. Lewin; "Temperature measurement" by J. M. Sturtevant; "Determination of melting and freezing temperatures" by E. L. Skau, J. C. Arthur, Jr., and H. Wakeham; "Determination of boiling and condensation temperatures" by W. Swietoslawski and J. R. Anderson; "Determination of vapor pressure" by G. W. Thomson; "Calorimetry" by J. M. Sturtevant; "Determination of solubility" by W. J. Mader, R. D. Vold, and M. J. Vold; "Determination of viscosity" by J. F. Swindells, R. Ullman, and H. Mark; "Determination of properties of insoluble monolayers at mobile interfaces" by A. E. Alexander; "Determination of surface and interfacial tension" by W. D. Harkins, revised by A. E. Alexander; "Determination of osmotic pressure" by R. H. Wagner and L. D. Moore, Jr. Although these chapters are retained from the previous edition, the following are new contributors to them: Lewin, Arthur, Mader, Swindells, and Ullman. The chapter on viscosity is essentially new and is a much more satisfactory presentation than that of the previous edition. Alexander's chapter on insoluble monolayers at mobile interfaces replaces "Properties of monolayers and duplex films" by W. D. Harkins. It is limited to a presentation of the properties of monolayers at air-water and oil-water interfaces and a discussion of the use of the former to investigate processes taking place at interfaces.

The remaining chapters, although they incorporate new topics, tables, figures, examples, and some new discussion of theory, are essentially unchanged. The average increase in number of references is 25 percent, and the same increase holds for the length of the chapters. The authors are to be commended for bringing their work up to date.

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Statistical Theory of Communication. Y. W. Lee. Wiley, New York, 1960. xvii + 509 pp. Illus. $16.75.

The modern curriculum in electrical engineering is distinguished from its predecessors by an emphasis on fundamental theory rather than on the details of "hardware." This is as it should be; fashions in electronic circuitry change so quickly that a concentration on detail rather than on principle would render any engineering education obsolete within a very few years. One of the major basic subjects to receive attention recently is the theory of communication. While there have been several texts on modulation theory, theory of noise, and information theory, all except Middleton's weighty volume are fairly specialized and are not suitable for a general introduction to the subject. In many ways this book fills the need for such an introductory text.

This book begins with an account of Wiener's theory of generalized harmonic analysis. The account, though heuristic, is well written and shows the author's thorough mastery of the subject. Good motivating arguments are given for the introduction of the autocorrelation function, spectral density, and related functions. The discussions are accompanied by numerous illustrative examples. The chapters on harmonic analysis are followed by several chapters on the theory of probability, but the latter are less well written than the former. With the current emphasis on the formulation of engineering problems in probabilistic terms, little less than a complete course in the subject can adequately cover the basic concepts.

The next several chapters, an excellent feature of Lee's book, are devoted to discussions of a number of the practical problems encountered in adapting "hardware" to make use of theory. The many pictures of experimental correlograms lend interest to the theoretical

discussion. Wiener's theory of optimum filters and some ramifications due to the author are then discussed. The criteria of fidelity are all of the mean-square-error type. The resulting Wiener-Hopf equations are discussed, *mirabile dictu,* without once mentioning analytic continuation. However, this is possible if one restricts the discussion to rational spectra. Within this limitation the author has done a fine job of presenting the elements of the theory of filtering and prediction. The final chapters are on the representation of correlation functions by orthonormal functions, a method pioneered by Norbert Wiener and the author.

It is regrettable that no account is given of at least the definitions of information theory. Otherwise this book has much to recommend it as an introduction to work in the theory of communication.

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Glossary of Atomic Terms. U.K. Atomic Energy Authority, London, 1960 (order from H.M. Stationery Office, London). 54 pp. 3s. 6d.

This pocket-size compilation contains about 400 brief definitions of chemical elements and compounds, reactor components, materials and accessories, instruments, and units of measurement frequently used in nuclear science and technology. Many of the entries are names and abbreviations of atomic and electric power plants in the British Commonwealth; for example, Calder Hall, HERMES, HIFAR, NRX—reactors in Great Britain (at Harwell), Australia, and Canada, respectively—and S.S.E.B.—South of Scotland Electricity Board.

The need for selectivity in a glossary of this size may account for some of the gaps, but hardly warrants the omission of relevant terms such as annihilation, atomic absorption coefficient, bubble chamber, californium, elementary particles, positronium, radiocarbon, nuclear magnetic resonance, and many others.

Nevertheless, this is a handy, inexpensive tool keyed to the needs of the nonspecialist who has some familiarity with science and technology.

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Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, but to the publisher or agency sponsoring the publication.)

Agricultural Problems in Arid and Semiarid Environments. Alan A. Beetle, Ed. Agricultural Experiment Station, Univ. of Wyoming, Laramie, 1960. 64 pp. A symposium held at the 35th annual meeting of the Southwestern and Rocky Mountain Division of the AAAS and the 30th annual meeting of the Colorado-Wyoming Academy of Science, 6–7 May 1959 at the University of Wyoming.

A Bibliography of Fossil Man. 1845-1955. George E. Fay, pt. 1. Southern State College, Department of Sociology and Anthropology, Magnolia, Ark., 1960. 100 pp.

Chemical Aspects of the Structure of Small Peptides. Dorothy Wrinch. Munksgaard, Copenhagen, Denmark, 1960. 194 pp. Kr. 24.

La Chimie electronique et ses applications industrielles. Andree Goudot. Presses Universitaires de France, Paris, 1960. 126 pp.

Drawings of British Plants. Stella Ross-Craig. Being illustrations of the species of flowering plants growing naturally in the British Isles. pt. 14, *Adoxaceae, Caprifoliaceae, Rubiaceae, Valerianaceae, Dipsacaceae*. Bell, London, 1960. 40 pp. 10s. 6d.

Educators Guide to Free Filmstrips. Mary Foley Horkheimer and John W. Diffor, Eds. Educators Progress Service, Randolph, Wis., ed. 12, 1960. 172 pp. $6.

The Electron. Alfred Bender. Sentinel Books, New York, 1960. 128 pp. $1.

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Reports

Nomenclature of the Nicotinamide Nucleotide Coenzymes

The nomenclature of the nicotinamide nucleotide coenzymes has been a subject of long-standing disagreement, and unanimity has not yet been reached. Several different systems are in use to varying extents.

Included with other subjects in the terms of reference of the Enzyme Commission of the International Union of Biochemistry (*1*) is the nomenclature of enzymes and of coenzymes, and during the past three years the commission has been giving careful and detailed consideration to the question of coenzyme nomenclature, in an effort to reach a generally acceptable solution. The question has also been considered by the Biological Chemistry Nomenclature Commission of the International Union of Pure and Applied Chemistry. A summary of the points involved and the reasons for the recommendations of the commissions may be of interest.

Present position. The situation that has to be faced is that, except perhaps in the United States, there is no unanimity about the naming of the two coenzymes. Four different systems are in use, although to very different extents, and the special difficulty of the position arises from the fact that the system that is most used is that to which there are the strongest objections from the chemical point of view. The four systems are (i) cozymase and phospho-cozymase, (ii) codehydrogenase I and codehydrogenase II (or codehydrase I and codehydrase II, terms used by some Continental writers), (iii) coenzyme I and coenzyme II (abbreviated to CoI and CoII), and (iv) diphosphopyridine nucleotide (DPN) and triphosphopyridine nucleotide (TPN). The first system is probably the least used, the fourth the most used, at present. None of these systems is satisfactory, the first three because they are uninformative, the last because it is incorrect, as a number of reviewers have pointed out (*2, 3*).

There is rather more consistency among the journals than there is among authors of books and reviews. The *Journal of Biological Chemistry*, which had previously used both cozymase and coenzyme I (systems i and iii), first used diphosphopyridine nucleotide (system iv) about 1940; it has continued to do so, along with cozymase and coenzyme I until about 1950, and as the sole form after that date. The *Biochemical Journal* used the names cozymase and coenzyme I, and did not permit use of diphosphopyridine nucleotide, until as recently as 1953, but since then it has used the last name almost entirely. The index of *Chemical Abstracts*, however, still uses codehydrogenase I (system ii) rather than DPN.

DPN, TPN. There would be no objection whatever to the use of the names diphosphopyridine nucleotide and triphosphopyridine nucleotide if they did not indicate chemical structures, but the main objection is, in brief, that not only do they fail to give the structure of the coenzymes properly, but they are the chemical names of other compounds. It is somewhat like using the name "methyl acetate" for pyruvate; the name indicates a structure, but it is the wrong structure.

The names are derived from, though not the same as, names introduced by Warburg (*4*) in 1936 as convenient descriptions. The term "pyridine nucleotide" was used, quite legitimately, to distinguish this class of compounds from purine nucleotides such as the adenine nucleotides. To distinguish the coenzymes from each other they were described as "the diphospho-" and "the triphospho-pyridine-nucleotide," at first with the definite article, but this was almost immediately dropped and the terms were thereafter used as names, in the German forms "Diphospho-Pyridinnucleotid" and "Triphospho-Pyridinnucleotid."

"Diphospho-." In 1939 F. G. Fischer (*2*), in his review of the subject, pointed out that these names, though concise, were incorrect. According to chemical terminology, "phospho-*X*" denotes *X* combined with an additional phosphate group; but a "nucleotide" already includes a phosphate group; thus a "diphospho-(di)nucleotide" would contain four phosphate groups, and not two as does the coenzyme. Fischer suggested that, especially in relation to the accepted names of the analogous flavin nucleotides, the names "Pyridine-Adenine-Dinucleotide" and "Phospho-Pyridine-Adenine-Dinucleotide" would be clearer. They would have the advantage of showing that the compounds are dinucleotides containing adenine, which was not indicated by the earlier names. Probably because of the outbreak of war in the same year, these suggestions did not receive the attention they deserved.

"Triphospho-." It was at first believed that the three phosphate groups of TPN were joined in line, as in ATP, so that the relationship between the two coenzymes would be analogous to that between ADP and ATP. When the true structure of TPN was determined in 1950 by Kornberg and Pricer (*5*), it was found to be a monophosphodinucleotide, so that the prefix "triphospho-" became less appropriate.

"Diphosphopyridine-." Perhaps the most serious objection arose when the names became Anglicized—first, I believe, about 1940 in the United States. The form used by Warburg, namely "Diphospho-Pyridinnucleotid," was clearly intended to be taken in the sense of diphospho (pyridinenucleotide), as shown by the hyphen and the capital P. The introduction of a space before "nucleotide" gave a form with quite a different meaning. The name "diphosphopyridine nucleotide" means, quite unambiguously, a nucleotide of diphosphopyridine, that is, a substance of the structure diphosphopyridine—D-ribose—phosphate, and it is not surprising that biochemists have often been criticized by the organic chemists for using such names.

"Dihydrodiphospho-." For the reduced or dihydro- forms, DPN and TPN yield names beginning with the undesirable prefix "dihydrodiphospho-," which is a further objection.

"Pyridine-." One may properly use the term "purine nucleotides" for the whole class, but in referring to a particular nucleotide the name of the particular purine is used. One says "flavin-adenine dinucleotide" and not "flavin-purine dinucleotide." Correspondingly,

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Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

the word "pyridine" ought not to be used for the names of particular nucleotides unless they are in fact nucleotides of pyridine itself. "Pyridine" should not be used when "nicotinamide" is meant. The coenzymes are nicotinamide compounds, and the corresponding pyridine compounds are inactive as coenzymes (*6*). The DPN system of nomenclature causes great difficulty in the rapidly expanding work on analogues of the coenzymes. If the nicotinamide nucleotides are called "pyridine nucleotides," what can one call the pyridine nucleotides? They surely cannot be called pyridine analogues of the pyridine nucleotides! The use of DPN forces the use in current literature of such terms as "pyridine-DPN" and APDPN (for acetyl-pyridine diphosphopyridine nucleotide), in which the word "pyridine" occurs twice, although there is only one pyridine ring in the compound. This is a very real difficulty.

"DNP." DNP is the accepted abbreviation for dinitrophenol, a reagent much used in studies on the coupling of biological oxidation and phosphorylation. Papers on DNP often contain many references to DPN, and the similarity makes them confusing and difficult to read.

"FMN, FAD, NMN,—?" Finally, the name diphosphopyridine nucleotide obscures the close and important analogy with the corresponding flavin compounds, shown in Table 1. Flavin-adenine dinucleotide (FAD) is the same substance, with the nicotinamide end changed into the flavin structure. Flavin mononucleotide (FMN) is the same without the adenylic acid half of the molecule. Nicotinamide mononucleotide (NMN) is the coenzyme without the adenylic acid. These three names and the corresponding abbreviations are quite generally accepted, and there is no suggestion that they should be changed. To complete the scheme we should expect NAD for nicotinamide-adenine dinucleotide, thus:

FMN	NMN
FAD	NAD

The use of DPN instead of NAD breaks the logic of the nomenclature.

Summary of objections. To recapitulate, the main objections to the name diphosphopyridine nucleotide are (i) the coenzyme is not a nucleotide of diphosphopyridine as the name indicates; (ii) the name indicates the presence of four phosphate groups instead of two; (iii) it does not indicate that the coenzyme is a dinucleotide or that it contains adenine; (iv) the coenzyme is not a nucleotide of pyridine but of nicotinamide; (v) the pyridine nucleotides, to which the name properly applies, are inactive as coenzymes; (vi) the use of the name for the nicotinamide compound makes it impossible to name the pyridine analogues satisfactorily; (vii) the combination "dihydrodiphospho-" in the name of the reduced form is undesirable; (viii) the name is out of line with the FMN, FAD, NMN sequence and obscures the close chemical analogy with the flavin compounds; (ix) the corresponding name triphosphopyridine nucleotide suggests a triphosphate structure rather than the actual monophospho-dinucleotide structure.

In view of the strong chemical objections, the Enzyme Commission felt itself unable to recommend the continued use of the DPN system of nomenclature, particularly as a satisfactory alternative is available. The difficulty is not a chemical one, for there is no doubt about what the substance is: it is nicotinamide-adenine dinucleotide. It is a psychological difficulty—that of bringing the correct name into use when an incorrect name has become well established. The commission fully realizes that it is taking a serious step in recommending a change, and that there is likely to be some opposition, perhaps especially in America, as there was in Britain when the form DPN replaced CoI. However, this moment, when a real effort is being made to bring the terminology of enzymology into order, is the right one for putting the coenzyme nomenclature right also. This is probably the last opportunity there will be for doing so; if it is not done now, there is not likely to be another chance.

Some workers feel that the whole matter should be left optional, so that authors should be free to use any system they wish. The difficulty facing the commission, however, was that, apart from the fact that they were given the task of dealing with coenzyme nomenclature, they also had to deal with the nomenclature of enzymes, and the names of a large number of enzymes depend on those of the coenzymes. They were therefore obliged to make a choice, in order to avoid the necessity for more than one systematic name for each of these enzymes.

NAD. They have therefore decided to recommend that both forms CoI and DPN be dropped, and that the coenzyme be known by its actual chemical name, nicotinamide-adenine dinucleotide, which may be abbreviated to NAD. It will be seen that this name avoids all the objections mentioned above, it indicates the structure of the compound, it brings out the analogy with FAD, and it makes it possible to name the analogues satisfactorily. The reduced form would be named dihydronicotinamide-adenine dinucleotide, which indicates correctly the part of the molecule that becomes reduced.

NADP. TPN (CoII) might be named phospho-nicotinamide-adenine dinucleotide, but this is undesirable, for "phospho-nicotinamide" is as incorrect as "diphosphopyridine," and the reduced form would again have the "dihydrophospho-" prefix. The recommended form "nicotinamide-adenine dinucleotide phosphate" (NADP) avoids both these objections, and the extra P clearly indicates the formation from NAD by the addition of a phosphate group.

Coenzyme analogues. With the NAD system, the naming of the analogues be-

Table 1. Structures of nicotinamide and flavin nucleotides.

Name and abbreviation	Structure
Nicotinamide mononucleotide (NMN)	Nicotinamide—D-ribose—phosphate
Cozymase	
Coenzyme I (CoI)	Adenine—D-ribose—phosphate
Diphosphopyridine nucleotide (DPN) Nicotinamide-adenine dinucleotide (NAD)	Nicotinamide—D-ribose—phosphate
Flavin mononucleotide (FMN)	isoalloxazine—ribitol—phosphate (flavin)
Flavin-adenine dinucleotide (FAD)	Adenine—D-ribose—phosphate / isoalloxazine—ribitol—phosphate
Phospho-cozymase	Phosphate
Coenzyme II (CoII)	Adenine—D-ribose—phosphate
Triphosphopyridine nucleotide (TPN) Nicotinamide-adenine dinucleotide phosphate (NADP)	Nicotinamide—D-ribose—phosphate

comes relatively simple and straightforward. For example, the pyridine analogue would be called pyridine-adenine dinucleotide, which might be abbreviated to PAD if desired. The deaminated form is at present called "desamino-diphosphopyridine nucleotide," but this term gives little idea of its nature, since the part of the molecule which is deaminated (the adenine) is not mentioned in the name. The obvious name for it is nicotinamide-hypoxanthine dinucleotide (NHD). The analogue of TPN in which the phospho- group is attached to the 3′-position instead of the normal 2′-position is at present called "3′-triphosphopyridine nucleotide," a name which surely cannot be taken as indicating only one phosphate group attached to the 3′-position. The new system would give the natural name nicotinamide-adenine dinucleotide 3′-phosphate, perhaps abbreviated to NAD3P. Other analogues should cause no great difficulty. Abbreviations for the analogues are not being officially suggested; these are merely given as personal suggestions to illustrate the great advantages of the NAD system.

Possible objections. It remains to consider whether the names now proposed for the coenzymes are in strict accordance with chemical terminology.

"Nucleotide." Can NMN and FMN strictly be termed nucleotides? If the definition of a nucleotide is restricted to substances that can be obtained from nucleic acids (that is, to purine and pyrimidine nucleotides), they cannot, because of the different bases present. Probably, however, nobody would wish to narrow the definition in this way, and it is much more reasonable to use the term to denote the chemical structure:

base—pentose—phosphate

Clearly NMN qualifies as a nucleotide.

The case of FMN is not so clear, for two reasons. In the first place, it contains not ribose but ribitol, so that the —CHOH— group in position 1 of the ribose is represented by a —CH_2— group in the flavin. However, the term "nucleotide" is also applied to the deoxyribonucleotides, in which the —CHOH— group in position 2 of the ribose is replaced by a —CH_2— group. It is not unreasonable, therefore, to make the term cover both modifications, although the change in the 1-position is the more important, since it prevents ring formation. In actual fact, the flavin compounds have been universally called nucleotides for well over 20 years, and there is no suggestion that their nomenclature should be changed.

In the second place, the term "flavin" includes not only the base but the ribitol as well. Therefore the term "flavin nucleotide" must be understood as referring to a nucleotide containing flavin, rather than a nucleotide "of" flavin in the sense in which NMN is a nucleotide of nicotinamide.

"Dinucleotide." Finally, can NAD and FAD be strictly termed dinucleotides? A purist might hold that the term implies that the two mononucleotides are linked in the same way as the nucleotides in nucleic acid, by a 3′-5′-linked phosphate group. However, it would seem reasonable to consider a compound formed by the simple union of two mononucleotides to be a dinucleotide, particularly since the two classes of dinucleotides differ only in the point of attachment of a single bond.

"Dipeptide" has been cited as an analogy where the term implies a definite point of linkage, but the analogy is not a valid one. A dipeptide is not formed by the union of two monopeptides, and the name is clearly based on different principles. A truer analogy would be "disaccharide," which is used for a compound formed by joining two monosaccharides, without respect to the point of attachment.

Substances of the FAD type have been called dinucleotides ever since their first discovery, and it is the only name available. It has probably appeared far more often in the literature in this sense than in the sense of the 3′-5′-linked compounds, and it would be unreasonable now to restrict its use to the latter type.

Conclusion. Such are the reasons which have led the Enzyme Commission of the International Union of Biochemistry tc recommend the use of NAD and NADP. The Biological Chemistry Nomenclature Commission of the International Union of Pure and Applied Chemistry has also decided, after considering the possible alternatives, to recommend these names in place of the existing systems. Preliminary experience has shown that, even by those whose first reaction is to express a preference for the retention of DPN, the new names are very quickly found to be attractive and satisfactory. It is hoped that when they become the official recommendations of both international unions, journals will give a lead in their adoption.

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11 October 1960

Sustained Swimming Speeds of Dolphins

Abstract. Observations of four large groups of dolphins suggest that they are able to swim at a sustained speed of 14 to 18 knots. The blackfish are able to maintain speeds of about 22 knots, and one killer whale seemed able to swim somewhat faster. This implies that the apparent coefficient of surface friction remains approximately constant for dolphins from 6 to 22 ft long, as is the case for rigid bodies.

Since Gray (*1*) called attention to the anomalously high speed of dolphins (or porpoises) in 1936 and presented the case of a dolphin clocked at 33 ft/sec (about 19.7 knots), there have been a number of reports of similar high speeds of swimming. However, very little information on the sustained speeds of which these animals are capable has been published. On the basis of the various assumptions made, the speeds reported indicate a work rate per pound of muscle between 5 and 10 times that measured for terrestrial mammals, including man and the horse. However, data for terrestrial mammals (*2*) show that such mammals, by going into oxygen debt, can sustain work rates up to 100 times the basal metabolic rate for very short periods of time, and there seems to be a good possibility that many of the observations reported were of dolphins doing the equivalent of a 100-yard dash.

The lack of information in the scientific literature on the sustained speed capability of dolphins prompted us to have observations made through the cooperation of the Matson Navigation Company and Alexander Anderson, navigating officer of the S.S. *Monterey*. Printed forms were provided the observers for recording the time and location of observations, the speeds and relative positions of ships and dolphins, and the duration of sightings. Also included were seven sketches of species likely to be encountered on the trip from California to Australia, together with common names and lengths at maturity.

Most of the observations were made in the Southern Hemisphere while the

ship was traveling at between 19.6 and 21 knots. Many observations of short duration were made on 1 to 30 dolphins at a time as they were swimming with the ship, but after about 2 minutes at these speeds they dropped behind; this suggested that to sustain such swimming speeds was beyond their capability. Four large groups of dolphins were observed in calm seas, swimming at 14 to 18 knots for periods of 8 to 25 minutes. Details of these observations are given in Table 1. In at least two of these sightings the dolphins never came close to the ship or seemed to be deflected from their course by the presence of the ship. These data support the conclusion that dolphins 6 to 8 ft long can swim at a sustained speed of about 18 knots.

William Von Winkle, of the U.S. Navy Underwater Sound Laboratory, reported (*3*) that a school of blackfish (probably *Globicephala melana*) had been observed circling a Navy vessel, which was cruising at 22 knots, for several days at a time. They would pass the ship, go way out in front, and go back in the wake to look for food. They were 12 to 15 ft long. On the basis of model similiarity, a dolphin of this size, twice the length of the smaller animals, should have a muscle weight (and presumed power output), per square foot of surface area, twice as great. If the coefficient of surface drag were constant, this would lead to a speed $2^{1/3}$ (= 1.26) times as great, since the propulsive power increases as the cube of the speed; 1.26 × 18 knots (the sustained speed of the smaller, common dolphin) equals 22.7 knots, which comes very close to the speeds maintained by the blackfish.

The one killer whale (*Orcinus orca*)—actually a dolphin—that was observed from the *Monterey* traveled at speeds of 20.6 to 30 knots. If we assume that it was of average length, it should have been about 3 times the length of the 5- to 8-ft dolphin. On the basis of model similarities, this killer whale should be able to swim $3^{1/2}$ times faster than the 18 knots, or 1.44 × 18, or 26 knots. This value is about the speed at which the whale swam around the ship but less than the 30 knots at which it approached the ship. After playing around the ship for 20 minutes at a sustained speed greater than the 20.6 knots at which the ship was sailing, the killer whale continued on its original course at about the speed of the ship.

We suggest that under the assumption of a constant ratio of muscle power to muscle weight, the sustained speeds that have been observed imply that the coefficient of surface friction remains approximately constant for dolphins from 6 to 22 ft long. It seems probable that the capacity of these mammals for sustained work can be estimated more closely than their capacity for short-term high rates of work. Such estimates, together with the speed data of Table 1, should provide an improved basis for calculating the value of the surface drag coefficient.

Table 1. Observations made from the *Monterey* on sustained swimming ability of dolphins.

Approx. No. of dolphins	Date (1958)	Speed of dolphins (kn)	Length of time at observed speed (min)	Position of ship	Speed of ship (kn)	Comments
50	2 Nov.	18	8	32°S, 176°W	19.6	Some dolphins rode the bow wave. Large and small dolphins in group. Smaller ones came closest to ship.*
500	25 Oct.	14–16	20	34°S, 158°E	19.5	Group was traveling in same direction as ship when it was overtaken and passed. Group was about 0.25 mile away at closest approach.*
200	31 Oct.	18	10	34°10′S, 167°05′E	21	Some of the dolphins approached side of vessel but stayed 200 yd off.*
200–300	31 Oct.	17–18	25	34°15′S, 167°45′E	21	Dolphins were sighted several miles ahead of ship in calm sea. They remained about 0.5 mile away from vessel but swam on same course as vessel.*
1	12 Oct.	20.6–30	20	03°30′S, 141°45′W	20.6	Killer whale (about 20 to 25 ft long) approached ship at about 30 kn, swam back and forth around bow, veered away at about speed of ship.

* Dolphins were white-sided and about 6 to 8 ft long.

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12 July 1960

Interpeduncular Nucleus and Avoidance Conditioning in the Rat

Abstract. Rats trained to make a jumping response to the onset of a visual stimulus lost the habit after damage to the interpeduncular nucleus of the midbrain. It was noted, however, that the majority of the operated animals showed perfect retention of the "fear" response to the conditioned stimulus.

Thompson and Massopust (*1*) have demonstrated that damage to the interpeduncular nucleus of the mesencephalon abolishes a previously established brightness discrimination in the rat. More recently it has been found that this nucleus also participates in mediating a kinesthetic discrimination as well as an auditory habit (*2*). Because of the paucity of anatomical, physiological, and psychological data related to the interpeduncular nucleus, its function in learning and retention is unknown. Some enlightenment was obtained in the study reported here, in which the effects of lesions in the interpeduncular nucleus on retention of an avoidance response were studied (*3*). Of particular interest was the finding that although damage to this nucleus profoundly disturbed the subsequent performance of the avoidance response, the fear-producing properties of the conditioned stimulus were spared.

Nineteen adolescent albino rats were trained to make a jumping response to the presentation of a visual stimulus. The apparatus, which consisted of an enclosed box made of Plexiglas, was located in a light-proof, sound-attenuated room. The box measured 6 inches on either side and was 10 inches high. The floor of the box was made into a grid composed of bronze rods. A space of ½ inch existed between the top of the walls and lid of the box.

This space permitted the rat to grip the edge of the walls with its forefeet and raise itself, thus escaping or avoiding the charged grid. The box was mounted between two 100-watt frosted light bulbs.

A trial consisted of a 5-second presentation of the light, followed by a combination of light and shock until the subject succeeded in making the appropriate response. If the subject made a conditioned response (gripping the edge of the walls during the initial 5-second presentation of the light), the light was immediately turned off and the shock postponed. Twenty to twenty-five trials were given daily, with an intertrial interval ranging from 30 to 150 seconds (mean, 90 seconds). Training was terminated when the subject reached the criterion of nine conditioned responses within a series of ten trials. Approximately 4 hours after learning, 12 animals were subjected to electrolytic lesions in the interpeduncular nucleus, while the remaining seven received only sham operations (controls). After a recovery period of 7 days, all subjects were required to relearn the avoidance response under the same conditions as those described in original learning. After the experiment, histological verification of the location of the lesions was accomplished (*4*). Details of the surgical and histological procedures may be found elsewhere (*1*).

Five of the 12 experimental animals had less than 20 percent damage to the interpeduncular nucleus. These animals achieved a mean trial savings score of 82 percent which is comparable to the retention performance of the controls (mean of 85 percent). That minimal damage to the interpeduncular nucleus has no effect on retention is in agreement with previous findings (*1*, *2*). The remaining seven experimental rats had lesions destroying from 35 to 85 percent of the nucleus; these rats earned an average savings score of −61 percent (Fig. 1 *A*). This retention score is significantly inferior to that of the controls beyond the .01 level. However, observations of these animals on the first day of the retention test indicated that some relevant memory of the conditioning situation was present. On the first trial of the retention test, five of the seven experimental animals made definitive "fear" responses to the presentation of the light prior to the onset of shock (see Table 1). These responses consisted mainly of squeaking, which was sometimes accompanied by abrupt changes in respiration and upward orientation of the head. In four rats, these fear responses elicited by the light occurred during every trial on the first day, but were never followed by a conditioned jumping response. These observations are not readily explicable on the basis of a motor disturbance. The unconditioned response (jumping in response to the onset of shock) exhibited by the experimental animals was executed as smoothly and with the same latency as that exhibited by the controls. Similarly, these data are not accountable in terms of a heightened sensitivity to the light (*5*). No disturbances in alertness or wakefulness were apparent in any of the experimental animals.

Table 1. Percent damage to the interpeduncular nucleus and performance scores for seven rats.

Rat No.	Damage (%)	Trials		Trial savings (%)
		Learning	Relearning	
3	85	20	60	−200*
10	35	53	95	−79*
15	45	74	61	18*
17	35	46	17	63
33	45	53	60	−13*
42	45	21	48	−129
79	55	43	81	−88*

* The animal exhibited fear responses on the first trial of the retention test.

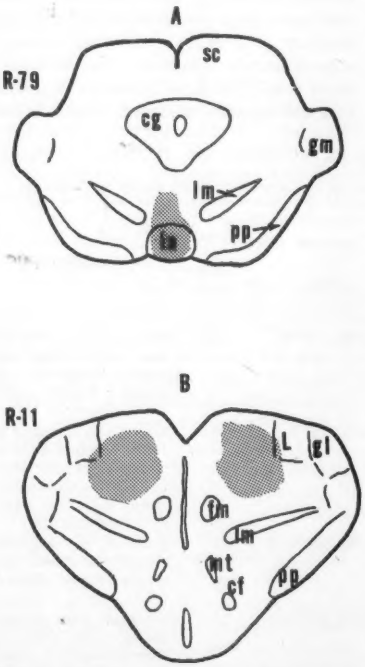

Fig. 1. Camera drawings of sections showing lesions (stippled area) in the interpeduncular nucleus (*A*) and the posterior thalamic region (*B*). Symbols: *cg*, central gray; *cf*, column of the fornix; *fm*, fasciculus retroflexus; *gl*, lateral geniculate nucleus; *gm*, medial geniculate nucleus; *in*, interpeduncular nucleus; *L*, lateral nucleus; *lm*, medial lemniscus; *mt*, mammillo-thalamic tract; *pp*, basal peduncle; *sc*, superior colliculus.

These observations are intelligible in terms of the two types of learning that take place in avoidance conditioning (*6*). The first type, frequently referred to as "visceral" conditioning, occurs early in training and is manifested by such responses as vocalization, urination, defecation, sudden motor jerks, and crouching. These responses evoked by the conditioned stimulus clearly reflect the animal's anticipation of pain. In the present experiment these anticipatory reactions were quite noticeable, in most cases, by the tenth learning trial. The second type of learning ("skeletal" conditioning) constitutes the adaptive response which effectually allows the animal to avoid the pain. This response, in the present experimental situation, became stabilized after about 50 trials. Presumably, lesions in the interpeduncular nucleus disrupt the neural pathways involved in this second type of learning, while sparing those pathways involved in the first type.

I subsequently endeavored to determine whether damage to other brain areas critical for visual performance would result in the same differential effect. The visual cortex was first investigated. Of the six rats with virtually total bilateral ablation of the visual cortical areas, three showed anticipatory fear responses to the onset of light on trial one. (One additional rat began exhibiting fear responses on trial two). However, these results are not altogether comparable to those obtained with lesions in the interpeduncular nucleus, since extirpation of the visual areas only partially impaired the conditioned jumping response. The animals with cortical damage earned an average-savings score of 49 percent. This score is significantly inferior to that of the controls ($p = .05$), but is significantly superior to the performance of the seven animals with lesions destroying at least 35 percent of the interpeduncular nucleus ($p = .05$).

Finally, attention was focused on the posterior thalamic area of the brain (see Fig. 1 *B*). This region of the thalamus medial to the lateral geniculate bodies has been found to be highly critical for visual discrimination performance (*1*). Ten rats with bilateral lesions in the posterior thalamus revealed a mean trial-savings score of −150 percent, a score which is very significantly inferior to that of the controls. Of these ten rats, only one showed anticipatory fear responses to the light during the early trials on the first day of the retention test. By the use of Fisher's exact probability test, the frequency of fear responses shown by these animals was found to be significantly less than that exhibited by the seven animals with moderate dam-

age to the interpeduncular nucleus ($p = .05$).

The foregoing results strongly support the notion that two different neurological systems function in the retention of avoidance conditioning. The neural system involved in visceral conditioning probably contains the posterior thalamic region as one component, but does not include the interpeduncular nucleus or the visual cortex. In contrast, all three anatomical structures studied in this report are implicated with the neural system involved in skeletal conditioning, particularly the interpeduncular nucleus and the posterior thalamic area.

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3. This study was supported in part by research grant M-2529 awarded to the Department of Psychology, George Peabody College, from the National Institute of Mental Health of the National Institutes of Health, U.S. Public Health Service.
4. I acknowledge the assistance of Rita Thompson for preparation of the histological materials.
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13 June 1960

Possible Explanation of Fluoride-Induced Respiration in Chlorella pyrenoidosa

Abstract. Low concentrations of sodium fluoride significantly increase oxygen consumption and total phosphorylated nucleotides in respiring *Chlorella pyrenoidosa*. Measurements of gas exchange at several *p*H values indicate that the stimulation is probably related to the undissociated hydrogen fluoride concentration in the suspending media.

Although fluoride has long been regarded as an inhibitor of respiration, recent investigations have demonstrated that it will also stimulate respiration. The stimulatory effect is usually produced at low concentrations. This phenomenon has been reported for algae (*1*), yeast (*2*), seedlings (*3*), and leaf tissue (*4*), but little interpretation has been offered as to the processes involved. The investigation reported here was an attempt to relate the fluoride-induced increase in oxygen uptake with some phase of the metabolism of *Chlorella*.

Chlorella pyrenoidosa Chick, Emerson strain type D, was grown at 24 to 26°C in a modified Knop's solution with added micronutrients. Iron was supplied as the salt of ethylenediaminetetraacetic acid. Air enriched to 3 percent CO_2 was continually supplied to the cultures. Unilateral illumination was provided by daylight-type fluorescent tubes, which supplied 600 ft-ca (6480 lux). After 72 hours' growth in constant light the cultures were subjected to 12-hour cycles of light and darkness for 48 hours and were harvested 6 hours after the last dark period.

The cells were harvested by centrifugation at approximately 500*g* for 15 minutes. The packed cells were resuspended in a volume of distilled, sterile water equal to the volume of packed cells. Cell counts were made on the resuspended algae, and all measurements were referred to these cell counts. Manometric measurements of the gas exchange in respiring *Chlorella* suspensions were made in 15-ml Warburg vessels in total darkness at 25°C. After a 110-minute treatment with NaF in 0.022*M* phosphate buffer at *p*H 4.0 it was apparent that $1.05 \times 10^{-4}M$ NaF produced an 8-percent increase in oxygen consumption and $1.05 \times 10^{-3}M$ NaF produced a 60-percent increase, while at $1.05 \times 10^{-2}M$ the rate was reduced to less than 70 percent of that of the nontreated system.

Experiments at other *p*H values, from 4.0 to 7.0, did not yield comparable results when the rate of oxygen consumption was compared to the total fluoride concentration of the solution. However, if the oxygen consumption was plotted relative to the calculated concentration of the undissociated HF, the results at different *p*H values were comparable (Fig. 1). The respiratory quotient did not vary greatly in the treated system as compared to the control.

To further define the mechanisms of fluoride-induced stimulation of oxygen uptake, the phosphate metabolism-respiration relationship was investigated. Respiratory-rate measurements were made on algae suspended in a nonbuffered water system in which the *p*H was initially adjusted to 4.0 with HCl or KOH. Similar suspensions of algae were placed in 40-ml test tubes that were attached to the manometer supports, and from these, aliquots were removed for phosphate determinations. Fluoride was added as sodium fluoride at zero time.

Measurements of phosphorylated nucleotides were made with a modification of the Norit A method (*5*). This procedure excludes sugar phosphates and other phosphorylated metabolic intermediates. The determinations of inorganic phosphate, after hydrolysis, were made according to the Fiske-Subbarow method (*6*).

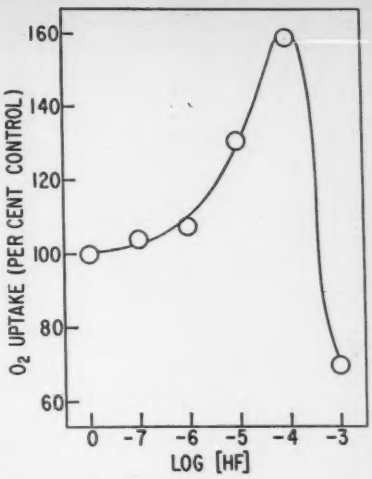

Fig. 1. Oxygen uptake in relation to the calculated HF concentration. The total fluoride concentration varied from 1.05×10^{-8} to $1.05 \times 10^{-1}M$ at *p*H 7.0, and from 1.05×10^{-4} to $1.05 \times 10^{-2}M$ at *p*H 4.0 in 0.022*M* phosphate buffer.

Concentrations of fluoride of 1.05×10^{-4}, 1.05×10^{-3}, and $1.05 \times 10^{-2}M$ increased oxygen consumption and at the same time increased the total phosphorylated nucleotides (Fig. 2). At $1.05 \times 10^{-4}M$ the oxygen consumption increased to 107 percent of that of the

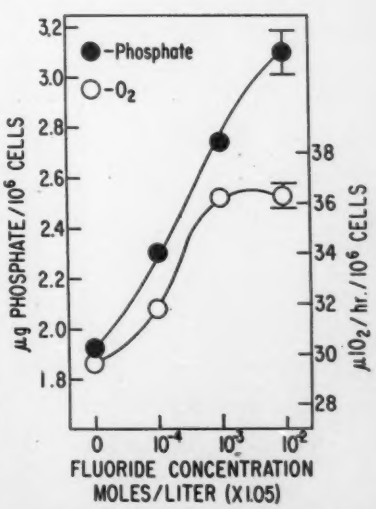

Fig. 2. Total nucleotide phosphate and rate of oxygen uptake in different concentrations of fluoride in the suspending solution. Undissociated HF concentrations were not calculated since this was a nonbuffered system. Bars at terminal points indicate 95-percent confidence limits.

control while the esterified phosphate increased to 119 percent. At a concentration of $1.05 \times 10^{-3}M$, oxygen consumption increased and esterified phosphate increased to 153 percent of that of the control. At the highest concentration reported here, $1.05 \times 10^{-2}M$, there was no further increase in the rate of oxygen consumption, but the esterified phosphate increased to 160 percent of that of the control.

The increase in esterified phosphate at fluoride concentrations above the concentration at which oxygen consumption fails to increase may indicate that fluoride was affecting multiple enzyme systems; this would be in accord with Reiner's (*7*) theoretical mechanism of multiple enzyme inhibition resulting in a stimulation of the oxygen consumption. The application of Reiner's theoretical treatment depends essentially upon the increased esterified phosphate's being adenosinetriphosphate (ATP).

Hackett (*8*) has indicated that the general over-all control of the rate of respiration in plants is dependent upon the concentration of the acceptor, adenosinediphosphate (ADP), rather than the donor, ATP. In addition, Krebs (*9*) has proposed that the control of respiration is dependent upon an interrelationship between inorganic phosphate, ADP, and ATP, ADP or phosphate acceptor being the most significant factor in the control mechanism.

Since there was a significant increase in phosphorylated nucleotides in these experiments, the following interpretation may apply. Fluoride probably disrupts the basic energetics of the cell in some manner and increases the oxygen consumption by increasing the phosphate acceptor or donor or at least by disturbing the interrelationships between inorganic phosphate, ADP, and ATP (*10*).

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23 June 1960

Packaged Organic Materials as Monitoring Tools for Radionuclides

Abstract. Pint-size perforated polyethylene bags were used as containers to test preserved tea, spinach, ion-exchange resin, live filamentous green algae, and dead filamentous green algae for the sorption and concentration of radionuclides from natural aquatic habitats and from a variety of laboratory controlled nutrient media. These packaged materials have been used to detect trace levels of radionuclides not found by the usual methods of analysis of the raw water itself for dissolved radionuclides.

In measuring nuclide radioactivity levels in the water environment at points downstream from nuclear-energy facilities, problems are encountered that require large-volume samples for analysis because of the dilution that has taken place. Even if the levels of specific radionuclides released are known, analysis of downstream media is necessary to determine the fate of the radioactive materials in the particular environment and to define parameters of dispersion and diffusion necessary for evaluating the movement of specific nuclides and the time required for their passage through the system in question.

In concentrating trace amounts of nuclides from large volumes of sample, particularly where evaporation, precipitation, or ion-exchange techniques are used, the stable salt concentrations in the diluting medium interfere with the subsequent separation of the specific radionuclides. To avoid these difficulties a technique utilizing dead organic and living biological concentration under natural stream conditions was investigated. Radioanalysis of algae from natural aquatic habitats has shown a greater variety and higher concentration of radionuclides than an analysis of the water in which the algae live.

Considerable data exist in the literature regarding concentration of fission products by organisms (*1*). Phytoplankton are noted for accumulating substances in inorganic form without known metabolic function. Average concentration factors up to 7000 have been reported for fission products (*2*), whereas some induced radionuclides have been concentrated several hundred thousand times or more. This characteristic is particularly noted in lower forms of organisms in media low in stable phosphorous. Thus, under certain circumstances, sampling by biological concentration may avoid the need of collecting large volumes of water for radioanalysis. Furthermore, knowledge of the movement of radionuclides into organic materials may reflect useful information necessary to evaluate the disposal of radionuclides into the hydrosphere.

High concentrations of potassium and calcium were found not to interfere, respectively, with the uptake of cesium-137 and strontium-85 by many nonliving organic materials in laboratory trials. Several kinds of biological material were selected for study under field conditions and in the laboratory, including preserved materials—tea, spinach, and filamentous algae—and living filamentous algae. Because the filamentous green alga, *Pithophora oedogonia,* grows easily in the laboratory and has a high concentration factor for many radionuclides, it was selected as the living test organism. It was grown free of silt and interfering radionuclides and in low concentration of stable nuclides. Thirty grams of blotted wet algae, about 2 g by dry weight, were placed in a pint-sized polyethylene bag having 400 evenly spaced pores about 0.8 mm in diameter. About 200 of these packaged sampling materials have been tested by

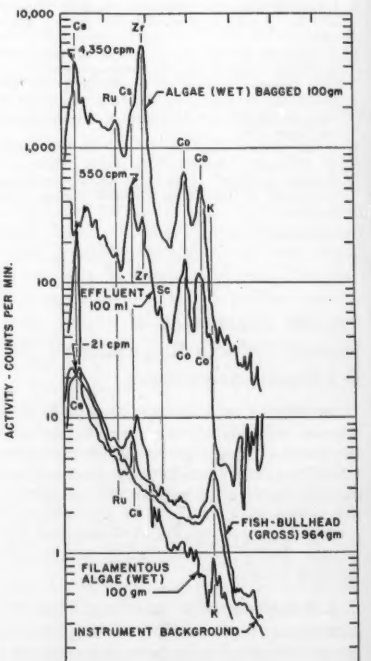

Fig. 1. Uptake of radionuclides from the Mohawk River, New York, at 17°C in September 1959 after 72 hours, by *Pithophora* in perforated polyethylene bags, compared with activity of fish (bullhead) and an indigenous filamentous green alga, *Cladophora,* taken from the river. Uptake of these radionuclides by *Pithophora* was nonmetabolic, since the alga had been killed with chlorine.

tying them submerged in the Ohio, Little Miami, Mohawk, and Clinch rivers for periods of 2 to 7 days, after which the contents showed high concentrations of several radionuclides, including cerium, cobalt, cesium, iodine, zirconium, ruthenium, zinc, and strontium. The presence of most of these radionuclides was not apparent from an analysis of the usual 1-liter volume of water. Packages of preserved tea, spinach, ion-exchange resin, and dead *Pithophora* showed less selectivity in uptake than live algae. In the dead group highest uptake occurred in dead *Pithophora*. Where aquatic environments are unfavorable for metabolism of live algae, these packaged materials could be used.

An advantage of sampling with such concentrators, particularly in the analysis of gamma-emitting radionuclides (all those listed above, except strontium), is that the contents of the polyethylene bag may be placed in a suitable container and counted directly without elaborate preparation of the sample, and sample preparation is made easier for strontium quantitation.

The chemical similarity of K to Cs and Ca to Sr may account, in part, for the ability of algae to concentrate all of them. However, these organisms assimilate only K and Ca. They do not substitute Cs for K or Sr for Ca. Therefore, the presence of nutrient levels of K and Ca are necessary for metabolism, which may permit the algae to accumulate huge amounts of nonmetabolic Cs and Sr. High levels of Ca and K, however, do reduce the accumulation of Sr and Cs (*3*). This has been repeatedly demonstrated in laboratory experiments. This reduction, however, is only apparent in live cells and has not been demonstrated in nonliving organic materials. For this reason preserved tea leaves, which are not as efficient as live *Pithophora* for concentrating Cs and Sr, may be better for quantitating fission products in natural bodies of water because this kind of uptake is not influenced, respectively, by natural levels of K and Ca. For example, at 25°C, preserved tea leaves were found to reach a peak equilibrium in 3 days and to concentrate Sr^{85} about 130 times and Cs^{137} about 80 times from water containing 3 or 30 parts of K or Ca, or both, per million, at several trace levels of these radionuclides. Experimental errors in the logs of counts were found to be approximately constant for this and some other organic material over the wide range of concentrations of these radionuclides, which might be similar to what could be expected from natural aquatic habitats.

Live *Pithophora* for quantitating is impractical, however, because moderate levels of K reduce the uptake of Cs and high levels of Ca reduce the uptake of both Sr and Cs. Both K and Ca concentrations vary widely in natural aquatic habitats; however, the uptake varies with the concentration of these competing ions; this fact may permit some generalizations.

For concentrating, live algae have a marked advantage over ion-exchange resins and dead materials, because they not only adsorb but concentrate ions such as Cs and Sr by active (living) transport into solutions with high concentrations within the cells. Resins can only absorb.

In the laboratory live *Pithophora* has concentrated Cs^{137} and Sr^{85} over 20,000 times. The efficiency of *Pithophora* as a biological indicator and concentrator under river conditions is shown by the gamma scan obtained with *Pithophora* (Fig. 1) compared to an indigenous filamentous alga (*Cladophora*) taken from the Mohawk River. With the exception of cerium, *Pithophora* shows the higher concentrations of these nuclides taken up by the two media and also demonstrates the presence of other radionuclides, notably cobalt. For further comparison, samples of fish and water taken at the same sampling location are included. Similar data, obtained with other organic concentrators immersed in the Clinch River at the mouth of White Oak Creek below the Oak Ridge National Laboratory, are shown in Fig. 2.

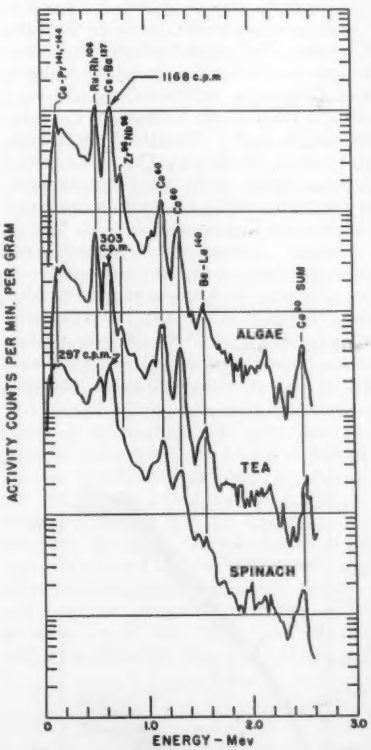

Fig. 2. Uptake of radionuclides in the Clinch River at the mouth of White Oak Creek near Oak Ridge National Laboratory, during 24 hours at 0°C in February 1960 by live *Pithophora*, preserved tea, and spinach packaged in perforated polyethylene bags. Uptake of these gamma-emitting nuclides by live *Pithophora* at this station at 20°C in June 1959 was generally higher by a factor of 10, while Cs^{137} was higher by a factor of 100, in laboratory trials.

The bulk of the information obtained, at least in the initial phases of evaluation, is qualitative, indicating the identity of the specific nuclides accumulated. Laboratory study indicates that quantification may be possible, because the concentration ratio for each medium at these trace levels, regardless of the nuclide, varies directly with the concentration of the nuclide in the water at the same temperature. Perhaps these concentrators may be useful in locating release of radioactive materials from waste containers disposed at sea.

Concurrent laboratory studies are in process in conjuction with field studies to identify, collect, and grow, under controlled conditions, selected biological concentrators.

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23 June 1960

Parental Body Build and Developmental Progress in the Offspring

Abstract. Children selected for study according to the body build of their parents were found to differ in rate of growth and in timing of osseous development. Boys and girls with large-chested parents were taller and heavier during the growing period and were more advanced in skeletal development than offspring of narrow-chested parents.

A relationship between "mesomorphic" body build and accelerated sexual maturation in boys has been reported by a number of workers (*1*). However, these findings are open to question because of the subjective nature and poor reliability of ratings on physique during childhood (*2*) and because greater

Table 1. Average comparative skeletal development of LL and SS boys and girls during infancy and childhood.

Variable	Sex	LL	SS	t
No. hand-wrist centers at 1.5 yr	M	7.8	6.3	1.73
No. hand-wrist centers at 1.5 yr	F	18.6	15.2	2.55
No. hand-wrist centers at 3.0 yr	M	22.1	18.4	4.20
No. hand-wrist centers at 3.0 yr	F	24.4	23.5	2.64
Hand-wrist completion (age)	M	6.7	7.7	2.05
Hand-wrist completion (age)	F	7.3	8.0	2.18
Bone age at 11.0 yr	M	11.9	10.1	5.29
Bone age at 11.0 yr	F	10.0	11.1	0.21

muscularity is a normal correlate of physiological advancement during the growing period.

In a preliminary investigation, subjects were selected according to parental mating combinations. The parents were categorized, by sex, according to their bony-chest diameters as measured on posteroanterior teleoroentgenograms, as "large" (above the mean) and "small" (below the mean). Children of the LL (large × large) and SS (small × small) mating combinations were then considered. In all, there were 20 LL parental mating combinations, with 56 offspring, and 15 SS mating combinations, with 31 offspring, in the study; however, the sample size was smaller in the adolescent period.

In replicate tests, the adult bony-chest diameter exhibited excellent short-term reliability ($r = 0.98$ to 0.99) and good 5-year reliability ($r > 0.90$) as an index of physical development. The bony-chest measurement has been shown to correlate well with the fat-free mass, or "lean body weight" (*3*), and is only slightly correlated with stature ($r = 0.2$). It is therefore a useful measure both of the lean body weight and of physique.

Offspring of the LL parental mating combination were compared with offspring of the SS combination for length and weight throughout the growing period, according to data, for individuals, from the Fels Longitudinal files. As shown in Fig. 1, LL boys surpassed SS boys in both length and weight from birth through 17 years, length being significantly greater from 5 through 13 years and weight from 1 through 17 years. The same tendency was observed for the girls, though the absolute differences were smaller, and significant only from 5 through 7 years for length and 5 through 9 years and at 17 years for weight. The 56 LL children of both sexes were longer and heavier than the 31 SS children throughout the growing period.

Further comparison was made for developmental status as measured by the number of hand-wrist ossification centers present at 1.5 and 3.0 years, the age of completion of the 28 bony nuclei of the hand and wrist (*4*) and for bone age at 11.0 years in both sexes, according to the Greulich-Pyle standards (*5*). As with length and weight, the LL children tended to be advanced over the SS children. More bony centers were present in the LL boys and girls at 1.5 and 3.0 years, and the full count of 28 centers was attained earlier in the LL children (Table 1).

A check on motor skills during early childhood showed LL children to be advanced over the SS offspring in Gesell scores at 0.5, 1.0, and 1.5 years (*6*), in Merrill-Palmer scores at 1.5 and 2.0 years (*7*), and in early Stanford-Binet quotients.

Clearly, parental body build, specifically the phenotypic mating combinations LL and SS, is associated with differences in the rate of growth and speed of maturation of the offspring. Children of broad-chested parents grow faster and are developmentally advanced during the growing period. Apparently, differences in adult physique are attained through different paths of development, suggesting that genes for body build also influence the rate of maturation (*8*).

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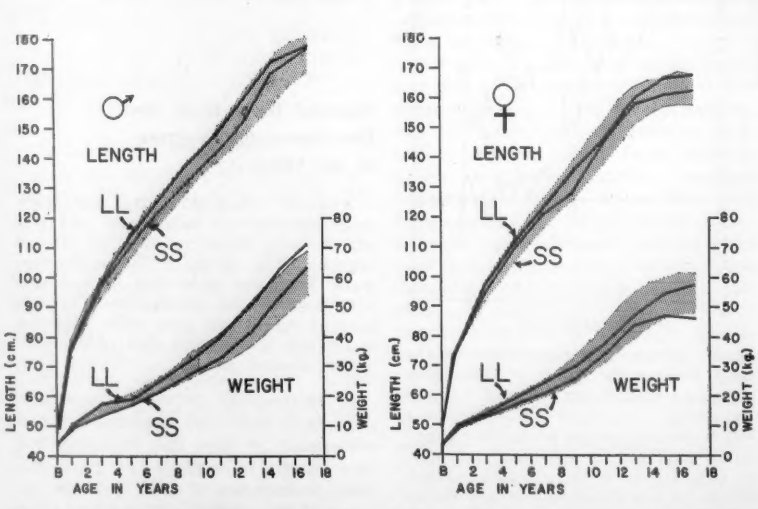

Fig. 1. Comparative growth of children of LL and SS parental mating phenotypes shown against the ± 1 σ limits (shaded areas) for the Fels Institute population. LL boys and girls tend to be longer during the growing period and heavier throughout.

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6 June 1960

Machine Retrieval of Pharmacological Data

The retrieval of pharmacological data from the literature has been reported by several workers in the field of science information. G. Congdon Wood (*1*) has devised a detailed code for storing, retrieving, and correlating chemical-biological data. Admittedly, the methodology of abstracting and filing

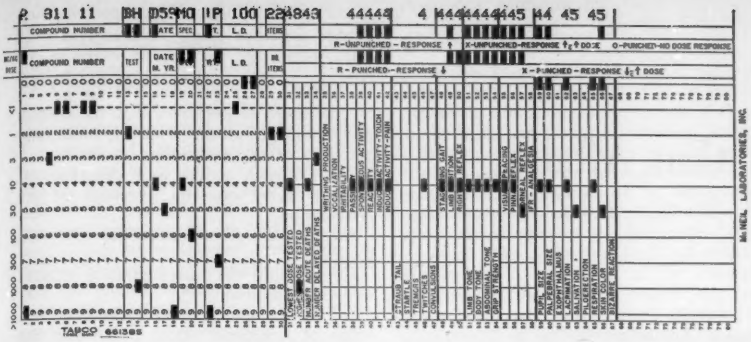

Fig. 1. Card punched with the results obtained from screening the action of chlorpromazine in mice.

is complex and covers most of the biological sciences. A unique system of indexing and searching pharmacological literature has been reported by H. E. Rockwell *et al.* (*2*) which frees the scientist from many hours of library searching. Isaac Welt (*3*) has developed the Cardiovascular Literature Project for collecting, classifying, and disseminating experimental and clinical information concerning the effects of chemical agents upon the cardiovascular system. Again, this is a rather complex system which involves journal abstracting and indexing the medical literature. All of these efforts are directed toward the published reports.

Our effort, on the other hand, is concerned with the raw data in the laboratory. With data retrieval becoming a problem of increasing magnitude which besets research laboratories dealing with a large volume of screening, it occurred to us that punched cards, with machine sorting, should serve as a research tool in making possible correlation of research far beyond that achievable by any amount of searching by hand. This report describes our experiences in the coding and recording on I.B.M. cards of a multiparameter screen —namely, a modification of the quantitative observational behavior assay as developed by Irwin *et al.* (*4*). It shows that the application of this idea to practice is entirely feasible.

Coded information was punched into a specially imprinted I.B.M. card. Each column represented a specific parameter or was used for general code such as compound number designation, type of screen, and species, as shown in Fig. 1.

The development of a code whereby punching could be accomplished required a knowledge and understanding of the data to be coded, a knowledge of the machine limitations for sorting, and anticipation of the type of questions to be asked of the cards. In general, the code adopted is based on the concept that a given activity is present at any given dose of a compound if this activity is still present at the succeeding dose. This concept eliminates a certain number of false data attributable to uncertainty of the observer. It may be seen from Fig. 1 that a logarithmically increasing dose scale appears on one edge of the card. Figure 1 also illustrates the various parameters employed in this screen.

Although each response is quantitated on an eight-grade scale on the original work sheet, we found it would not be feasible to attempt to record the magnitude of response of all of these parameters on the I.B.M. card. We have, therefore, resorted to the concept mentioned above. This provides what amounts to estimation of a minimal effective dose. The three zone punch areas of the I.B.M. card (*R*, *X*, and *O*) are used, as designated in Fig. 1.

After the source data had been translated to the cards, it was found advisable to challenge the code with a representative sample of cards. For example, we asked for a list of all the compounds in the sample which caused stimulation of the central nervous system at 1.0 mg/kg but which did not produce convulsions at 100 mg/kg, or less, when injected intraperitoneally in the mouse. By such challenges it was possible to discover and correct the inevitable mistakes that occur in the formative stages of developing a satisfactory retrieval system.

As stated above, we do not consider machine retrieval of screen data a replacement for a file system when it is desired to bring together all of the known information on a known compound. In the latter case, a numerical file is probably more satisfactory. Machine retrieval, however, is a very useful research tool and timesaver. We consider it important, since the most expensive part of data processing is the transcribing of source data to cards; a responsible individual with adequate scientific knowledge is required for intelligent coding and transcription.

Limitation of space prevents discussion here of the method used for transfer of data from the source document to the card. We feel that it is a mistake to code the investigator's judgment of the activity of a compound, since the interpretation of data changes with the development of new drugs. We consider it much more useful and of more lasting value to code, as nearly as possible, the raw data (*5*).

E. V. DIETRICH

McNeil Laboratories, Inc.,
Philadelphia, Pennsylvania

References and Notes

1. G. C. Wood, *Am. Document.* **8**, 168 (1957).
2. H. E. Rockwell, R. L. Hayne, E. Garfield, *Federation Proc.* **16**, 726 (1957).
3. I. D. Welt, Proc. Am. Chem. Soc. Div. Chem. Lit., 132nd meeting, New York, September 1957.
4. S. Irwin, M. Slabok, P. L. Debiase, W. M. Govier, *Arch. intern. pharmacodynamie* **118**.
5. I welcome correspondence with investigators interested in machine retrieval of data. I wish to thank Dr. William M. Govier for his valuable guidance and advice in the preparation of this report.

6 July 1960

Association Affairs

AAAS Constitutional Amendments

Two major amendments to the AAAS constitution, plus three more minor amendments made necessary by the two major ones, will be presented to the Council for action at the 1960 meeting.

History

At the 1959 meeting of the AAAS Council, the Committee on Council Activities and Organization recommended a number of changes in Council organization and activities. After substantial debate, Council approved most of the committee recommendations—namely, those portions which recommended (i) codification of the rules governing Council meetings (including some new rules of procedure); (ii) appointment of *ad hoc* study committees to review and to report to Council on such problems as were of interest to Council; and (iii) creation of a standing Committee on Constitution and Rules. Council did not approve the creation of a standing Committee on Council Activities and Organization, but voted to continue the committee of that name through 1960 to enable it to work further on the problems with which it had been concerned. A fuller discussion of the committee report and the actions taken by Council was published in *Science* [**131**, 504 (19 Feb. 1960)].

During 1960 the Committee on Council Activities and Organization has worked with the Board of Directors in an effort to clarify the functions of the Council and the Board, and the relations between the two bodies, and to define the composition and responsibilities of a standing committee to be called the Committee on Council Affairs. The committee and the Board of Directors have agreed upon the following amendments to the constitution. These amendments are now published so that they can be considered by Council and can be voted upon at the meeting of Council in New York in December.

Major Amendments

Article IV, Section 1

This section now reads: "The Council shall perform duties prescribed in the constitution and bylaws and shall act as an advisory body in matters pertaining to the general policies of the Association."

It is recommended that the following description of the responsibilities of Council be substituted for the above statement.

Responsibility for the affairs of the Association is vested in the Council, which shall have authority to delegate functions to the Board of Directors.

In addition to other powers and responsibilities set forth herein, Council shall:

(*a*) determine the rights and privileges of members, and prescribe the dues and fees to be paid by members;

(*b*) elect general officers of the Association;

(*c*) provide for the organization of the Association in sections in accordance with the fields of interest of its members;

(*d*) authorize the establishment of regional divisions of the Association;

(*e*) determine the conditions of affiliation and elect affiliates of the Association;

(*f*) establish special study committees to report to the Council on any aspect of Association policy or program or on other matters affecting the advancement of science; and

(*g*) establish committees to aid the Council in the discharge of any of the above responsibilities.

The proposed amendment would bring together in one place statements of the major functional responsibilities of Council and, together with the following amendment, would define more carefully the relationships between Council and Board.

Article V, Section 1

This section defines the responsibilities of the Board of Directors. It now reads: "The Board of Directors is the legal representative of the Association and as such shall have, hold, and administer all the property, funds and affairs of the Association."

It is recommended that this section be deleted and that the following statement be substituted. The new wording brings together from several places in the constitution statements of the major functional responsibilities of the Board.

The Board of Directors shall have, hold, and administer all the property, funds, and activities of the Association. It shall:

(*a*) take such actions as it deems necessary to carry out the purposes of the Association;

(*b*) make recommendations to the Council;

(*c*) appoint the administrative officers of the Association;

(*d*) in the event of a vacancy in the office of a general officer other than the office of president-elect, elect a replacement for the remainder of the unexpired term. In the event of a vacancy in the office of president-elect, replacement shall be by election by the Council;

(*e*) appoint and supervise the work of all committees except those described in Article IV, Section 1, paragraphs (*f*) and (*g*);

(*f*) determine the time and place of meetings of or meetings sponsored by the Association;

(*g*) elect Fellows from among the members of the Association;

(*h*) prepare and publish an annual budget for the Association and arrange for an annual independent audit of the accounts of the Association; and

(*i*) perform such other functions as the Council may assign to it from time to time.

Compensatory Changes

Each of the above amendments brings together into a single list a number of statements of responsibility that are now scattered through other articles of the constitution. In most cases it seems desirable to allow the duplication to remain. For example, Article VIII specifies that affiliates shall be elected by Council. The proposed amendment to Article IV includes the election of affiliates among the responsibilities of Council. Each article is clearer and more complete if the statement of Council responsibility appears in both.

In two cases, however, there will be simplification if the duplication is removed, and in one case a correction will be required if the two major amendments are adopted. These proposed changes are as follows:

1) In Article III, Section 2, delete the final sentence dealing with the filling of a vacancy in the office of a vice president. The problem will be adequately cared for in the revised Article V, Section 1, part (*d*).

2) In Article V, Section 2, delete the sentence beginning, "In the event of a vacancy in the office of an elected member of the Board of Directors. . . ." The method of filling such a vacancy is simplified and the procedure defined in the proposed Article V, Section 1.

3) Revise Article V, Section 4, by adding the italicized words so that the section will read: "The Board of Directors shall appoint such committees, *other than those described in Article IV, Section 1, parts (f) and (g),* as may be necessary to aid in the management of the Association. The duties of standing committees shall be stated in the bylaws." This change will remove a possible conflict in responsibility between the Council, which, if the recommended amendments are adopted, will have authority to appoint committees to deal with matters of concern to Council, and the Board, which according to the unrevised wording is responsible for the appointment of all committees.

Action by Council

The five amendments described above will be submitted to the Council for vote at the 1960 annual meeting in New York. A favorable vote of two thirds of the Council members who are present and voting is required for adoption.

The following recommendations are not for changes in the constitution but are related to the above proposed constitutional amendments. A favorable vote of a majority of the Council members present and voting will be required for their adoption. "Adoption" in the case of all of the following except the recommendation concerning the Committee on Constitution and Rules, will constitute a request to the Board of Directors to make the indicated amendments to the bylaws.

Committee on Council Affairs

As stated above, Council in 1959 voted against the creation of a standing Committee on Council Activities and Organization but did vote to continue the committee of that name for one year. The committee has worked with the Board of Directors in preparing the above recommendations and in redefining the nature and function of the standing committee that Council did not approve last year. The name of the proposed standing committee has been changed to Committee on Council Affairs. The *ad hoc* committee and the Board of Directors agree upon the desirability of establishing the proposed Committee on Council Affairs, and recommend to Council that the following statement of its composition and responsibility be approved. If Council approves, the statement will be formally adopted by the Board of Directors, which has authority to change the bylaws, as a new Section 11 of Article V of the bylaws, the section that lists and describes the Association's standing committees.

Proposed Addition to Article V of Bylaws

Section 11. The Committee on Council Affairs shall consist of: nine members elected by the Council for terms of three years each, the terms of three of whom shall expire on January 14 of each year; the president-elect of the Association, who shall serve as chairman of the committee; and the secretary of the Council, who shall serve as secretary of the committee without vote. The committee shall (*a*) prepare the agenda for meetings of the Council, (*b*) receive or initiate, coordinate, and advise on reports of Council committees, resolutions, or actions submitted for consideration by the Council, (*c*) establish or recommend to the Council the establishment of appropriate study committees to report to the Council on any aspect of Association policy or program or on other matters affecting the advancement of science, (*d*) recommend to the Council appropriate changes in the constitution and bylaws.

Other Changes in Bylaws

1) If Council approves the above recommendations, the Board of Directors will amend Article V, Section 1, of the bylaws in order to bring it into agreement with the other changes being proposed. The revision can be made by adding the italicized words to make the section read: "The committees shall be standing, as provided in the bylaws, or special, as the Board of Directors *or Council* approve(s). All standing committees, *except the Committee on Council Affairs,* shall report annually in writing to the Board of Directors. The chairmen of committees, *unless otherwise provided for in these bylaws,* shall be designated by the Board of Directors."

2) Already approved by Council at the 1959 meeting were several rules of procedure for Council meetings. These rules have been adopted by the Board of Directors as Article XII of the bylaws. No further action is required.

Committee on Constitution and Rules

At the 1959 meeting, Council approved the recommendation of the Committee on Council Activities and Organization that there be established a standing Committee on Constitution and Rules. The Committee on Council Activities and Organization and the Board of Directors, upon further consideration, agree that it is not desirable to have a standing Committee on Constitution and Rules. It seems better to continue to allow both Board and Council to have the authority to initiate recommendations for changes in the constitution and bylaws and to appoint a special committee for this purpose only when major revisions are required. It is therefore recommended that Council rescind the action it took in 1959 in voting to establish a standing Committee on Constitution and Rules.

Procedure for Amending Bylaws

Article XIII of the bylaws now specifies that "the bylaws may be amended by majority vote of the Board of Directors, provided notification of the proposed amendment has been mailed to each member of the Board at least twenty (20) days prior to the meeting." The Committee on Council Activities and Organization recommends that authority to amend the bylaws be transferred from the Board of Directors to the Council. If the Council votes to transfer authority to amend the bylaws from the Board to the Council, the Board will make that transfer by adopting the following statement as a substitute for the wording of Article XIII given above: "The bylaws may be amended by majority vote of the Council at any meeting or by mail ballot."

DAEL WOLFLE
American Association for the Advancement of Science

Programs Planned for the AAAS New York Meeting

Section and society programs in engineering, agriculture, and industrial science are presented here, and programs of some organizations not affiliated with any section. Programs in mathematics, physics, chemistry, astronomy, geology and geography, the biological sciences, anthropology, psychology, the social and economic sciences, medicine, dentistry, pharmacy, and the history and philosophy of science have been previously announced [*Science* **132**, 1259 (28 Oct. 1960); **132**, 1318 (4 Nov. 1960); **132**, 1403 (11 Nov. 1960); **132**, 1501 (18 Nov. 1960)].

Engineering

Section M. Panel, joint program of Section B–Physics and M–Engineering, cosponsored by the Society for the History of Technology: "The Place of Nuclear Engineering in the University Curriculum," arranged by Clarence E. Davies, United Engineering Center Project, New York City, with John W. Healy, General Electric Company, as moderator; 30 Dec.

Tau Beta Pi Association. The Association will hold its annual address with the AAAS, with Paul E. Klopsteg, retiring president, AAAS, presiding; 29 Dec. After an introduction by Donald

A. Dahlstrom, the speaker, Walter Lee Cisler, president and director of The Detroit Edison Company, will be introduced by Klopstek. The subject of the address is "The Increasing Significance of Energy in an Expanding World."

The Engineering Manpower Commission is a cosponsor of the program of the Conference on Scientific Manpower: "Developing Student Interest in Science and Engineering," with Samuel Schenberg, New York City Board of Education, presiding; 27 Dec. (For details, see the program of the Conference in the forthcoming issue of 2 Dec.)

The Engineers Joint Council is a cosponsor of the symposium "The Sciences in Communist China." Part II, "Meteorology and the Engineering Sciences," will be held on 26 Dec. Papers will be presented on an introduction to engineering and electrical engineering (T. C. Tsao, Columbia University); civil engineering (Lewis L. T. Au, Amman and Whitney Consulting Engineers, New York); mechanical engineering (Edward K. Nieh, Ebasco Services, New York); chemical engineering and metallurgical and mining engineering (L. C. Pan, Chemical Constructions Corporation, New York); electronics and computing, aeronautical engineering (Yao T. Li, Massachusetts Institute of Technology).

Agriculture

Section O. Six-session symposium: "Land-Zoning in Relation to Agricultural, Suburban, Industrial, Forest, and Recreational Needs of the Future," arranged by Firman E. Bear, Rutgers State University; 27, 28, 30 Dec.

Part I: "Rural Land Zoning," with Wallace D. Bowman, Conservation Foundation, presiding; 27 Dec. After an introduction by Bowman, the following papers will be presented: "What is Happening to Our Land" (Donald A. Williams, Soil Conservation Service, U.S. Department of Agriculture); "Principles of Rural Land Zoning" (Erling D. Solberg, Agricultural Research Service, U.S. Department of Agriculture); "Creating Permanent Agricultural Reserves" (Karl J. Belser, Santa Clara County, Calif., Planning Department); "Tax Deferrals for Land Withheld from Development" (T. W. Schulenberg, Indiana Department of Commerce and Public Relations).

Part II: "Suburban Planning," with Louis Wolfanger, Michigan State University, presiding; 27 Dec. After an introduction by Wolfanger, papers will be presented on subdivision control (Hugh R. Pomeroy, Westchester County Department of Planning, White Plains, N.Y.); soil-survey information for suburban development (Lindo J. Bartelli, Soil Conservation Service, U.S. Department of Agriculture); new concepts on suburban development districts (Marion Clawson, Resources For the Future, Inc.); regulating flood-plain development (Gilbert F. White and Robert Kates, University of Chicago).

Part III, joint program of Sections E, K, O, and P and the Association of American Geographers: "The Urban Frontier, A Conquest of Inner Space," arranged by Firman E. Bear (Rutgers State University), Frank C. Whitmore, Jr. (U.S. Geological Survey), and Charles C. Morrison, Jr. (American Geographical Society), with James E. Lash (Action, Inc., New York), presiding; 28 Dec. Papers will be presented on application of census statistics to problems of urban renewal (A. Ross Eckler, U.S. Bureau of the Census); urban renewal and metropolitan affairs (Martin Millspaugh, deputy general manager, Charles Center, Baltimore, Md.); two basic issues in local renewal policy (Coleman Woodbury, University of Wisconsin); renewal for industry—the opportunity and the problem (Dorothy A. Muncy, Consulting City Planner, Arlington, Va.); Newark, New Jersey—a case study in urban renewal (Paul Busse, Newark Economic Development Committee); urban renewal in New York City (George M. Raymond, Pratt Institute).

Part IV: "Community Planning," with Byron E. Munson, Ohio State University, presiding; 29 Dec. After an introduction by Munson, papers will be presented on small-city community planning (Herbert H. Smith, Community Planning Associates, Trenton, N.J.); county and regional planning board cooperation (George H. Deming, Conference on Metropolitan Area Problems, New York); possibilities in rural planning (Sanford S. Farness, Tri-County Regional Planning Commission, Lansing, Mich.); geologic contributions to community planning (William J. Wayne, Department of Conservation, Indiana Geological Survey). After presentation of the papers, the AAAS-Campbell Award will be presented.

Part V: "Forest and Recreational Planning," with Edward Higbee, University of Delaware, presiding; 29 Dec. After an introduction by Higbee, papers will be presented on biological requirements of man (Paul B. Sears, Yale University); conservation of open spaces (Charles W. Eliot (Cambridge, Mass.); program planning for national forest recreation (Edward C. Crafts, Forest Service, U.S. Department of Agriculture); development of water and forest preserves (Raleigh Barlowe, Michigan State University).

Part VI: "Government As Land Owner and Redistributor," with William Miller, New York University School of Law, presiding; 30 Dec. After an introduction by Miller, papers will be presented on conservation easements for securing open space for urban America (Max S. Wehrly, Urban Land Institute); law of open space (Shirley Adelson Siegel, Assistant Attorney General, State of New York); British town and county planning act (Ernest Weissman, Bureau of Social Affairs, United Nations); governmental assistance in area development (Robert E. Lowry, Tennessee Valley Authority).

Industrial Science

Section P. There will be an Industrial Science Award dinner, with Allen T. Bonnell, Drexel Institute of Technology, presiding; 29 Dec. The Section P luncheon meeting and vice-presidential address will be held on 30 Dec., with J. A. Hutcheson, Westinghouse Electric Corporation, Pittsburgh, presiding. The speaker will be Earl P. Stevenson, retiring vice president for Section P.

Institute of Management Sciences. Symposium, joint program with Section P: "Management Science," arranged by Gifford H. Symonds, Case Institute of Technology, with Roger R. Crane, Touche, Ross, Bailey and Smart, New York, presiding; 30 Dec. Papers will be presented on application of prediction techniques to management science (Max Woodbury, New York University); scheduling activities with uncertain demand (Gifford H. Symonds, Case Institute of Technology); a model of a competitive market (George Feeney, General Electric Company, New York).

Science in General

American Association of Scientific Workers. Round-table discussion: "Obstacles to the Application of Science for Human Welfare," arranged by Miriam L. Yevick, Adelphi College, who will preside; 27 Dec. Speakers will be C. Wright Mills, Columbia University, Otto Nathan, New York University, and Theodore Rosebury, Washington University School of Dentistry.

There will be a Dutch treat cocktail party on 27 Dec.

American Council on Women in Science. Third Conference on Women in Science, joint program of the American Council on Women in Science and Sigma Delta Epsilon; 27 and 30 Dec. The opening session will be an address: "Utilizing Our Scientific Womanpower"; 27 Dec. This will be followed by two concurrent panel discussions: panel A, "Changing Educational Trends," with John R. Cortelyou, DePaul University, as moderator; panel B, "Changing Cultural Trends," with Margaret Mead, American Museum of Natural History, as moderator; 27 Dec.

There will be a luncheon for women and an address (a program of the AAAS

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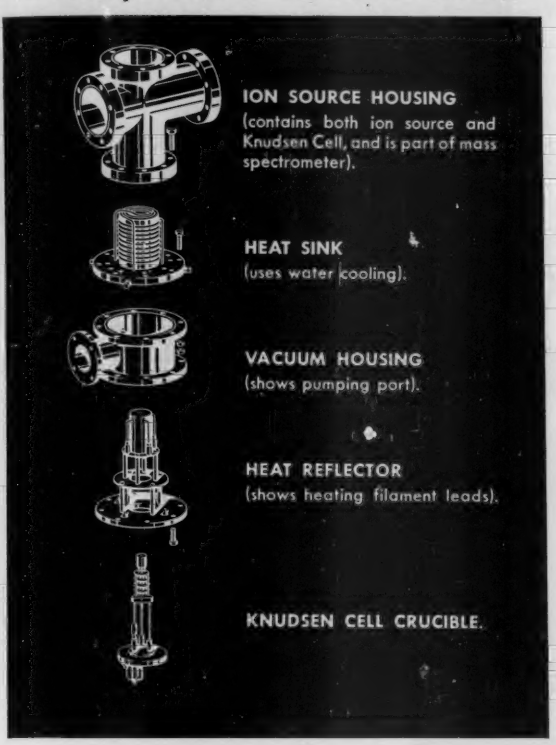

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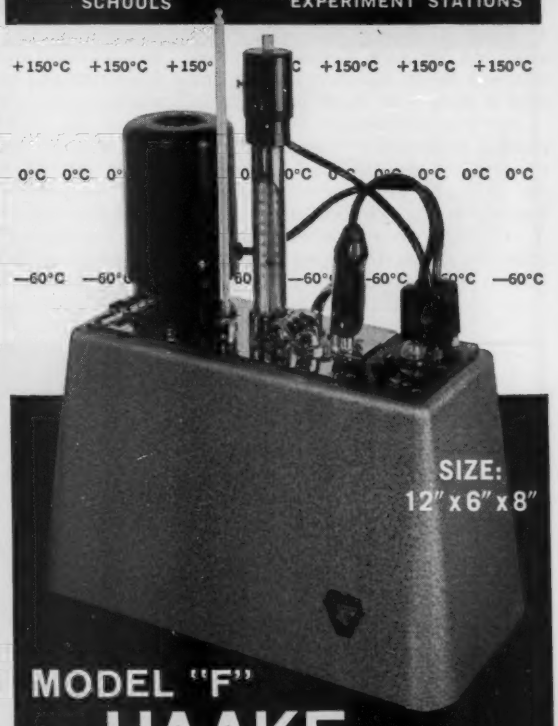

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Local Committee on Women's Events), arranged by Mrs. Eunice Thomas Miner, New York Academy of Sciences, who will preside; 30 Dec. The address, by Edith H. Quincy, College of Physicians and Surgeons, Columbia University, will be on radiation hazards and what is being done about them.

American Geophysical Union. Symposium, cosponsored by Section D–Astronomy and the American Astronomical Society: "The Impact of Space Research on the Sciences," arranged by the Planning Committee on Planetary Science of the AGU, Homer E. Newell, National Aeronautics and Space Administration, chairman, with Robert Jastrow, National Aeronautics and Space Administration, presiding; 26 Dec. Papers will be presented on the interaction between the earth sciences and planetary studies (Gordon J. F. MacDonald, University of California, Los Angeles); planetary environments and extraterrestrial life (Philip Abelson, Carnegie Institution of Washington); flying telescopes (Martin Schwarzschild, Princeton University).

Scientific Research Society of America. The Society will hold its annual convention on 29 Dec. On the same day there will be a joint luncheon of the Society of the Sigma Xi and the Scientific Research Society, and the annual address of the Scientific Research Society will be presented, with W. J. Coppoc, Texaco, Beacon, N.Y., presiding. Coppoc will award the William Procter Prize and Alan T. Waterman, National Science Foundation, will speak. The address is open to all who are interested.

Sigma Delta Epsilon. Cosponsor of the Third Conference on Women in Science. (For details, see the program of the American Council on Women in Science.)

There will be a National Council and Board of Directors meeting, with Ethaline Cortelyou, president of Sigma Delta Epsilon, presiding; 27 Dec.

On 28 Dec. there will be a luncheon for all women in science and an address, with Ethaline Cortelyou presiding. The address, "Petroleum—A Catalyst for Progress," will be given by Dorothy Quiggle, Pennsylvania State University.

On 29 Dec. there will be a dinner and grand chapter meeting.

Attention is called to the luncheon and program of the AAAS Local Committee on Women's Events, on 30 Dec. (For details, see the program of the American Council on Women in Science.)

Society of the Sigma Xi. There will be a joint luncheon with the Scientific Research Society of America, 29 Dec. (For details, see the program of the Scientific Research Society of America.)

The Society of the Sigma Xi will hold its 61st annual convention on 29 Dec. The joint address of the Society of the Sigma Xi and the United Chapters of Phi Beta Kappa will be given on the same day, with Mina S. Rees, member of the AAAS Board of Directors, presiding. The address, by Polykarp Kusch, Columbia University, will be on 'Scientists and Laymen."

Forthcoming Events

December

5–8. American Soc. of Agronomy, annual, Chicago, Ill. (L. G. Monthey, ASA, 2702 Monroe St., Madison 5, Wis.)

7–13. American Acad. of Optometry, San Francisco, Calif. (C. C. Koch, 1506–08 Foshay Tower, Minneapolis 2, Minn.)

9–10. The Myocardium—Its Biochemistry and Biophysics, New York, N.Y. (A. P. Fishman, New York Heart Assoc., 10 Columbus Circle, New York 19)

9–11. American Psychoanalytic Assoc., New York, N.Y. (D. Beres, 151 Central Park West, New York 23)

10–11. Academy of Psychoanalysis, New York, N.Y. (J. H. Merin, 125 E. 65 St., New York 21)

11–14. Hot Laboratory and Equipment Conf., 8th, San Francisco, Calif. (J. R. Lilienthal, Los Alamos Scientific Laboratory, P.O. Box 1663, Los Alamos, N.M.)

12–14. American Nuclear Soc. (Isotopes and Radiation Div.), San Francisco, Calif. (O. J. Du Temple, ANS, 86 E. Randolph St., Chicago 1, Ill.)

12–14. Water Pollution, natl. conf., Washington, D.C. (F. A. Butrico, Office of Engineering Resources, Div. of Engineering Services, U.S. Public Health Service, Washington 25)

12–16. Atomic Industrial Forum, conf., San Francisco, Calif. (D. J. Scherer, 3 E. 54 St., New York 22)

13–15. Eastern Joint Computer Conf., New York, N.Y. (E. C. Kubie, EJCC, Computer Usage Co., Inc., 18 E. 41 St., New York 17)

19–20. Statistical Mechanics, conf., London, England. (Organizing Secretary, Physical Soc., 1, Lowther Gardens, London)

22–2. Panamerican Diabetic Congress, 1st, British Honduras. (B. R. Hearst, Director, Diabetic Inst. of America, 55 E. Washington St., Suite 1646, Chicago 2, Ill.)

26–30. Inter-American Cong. of Psychology, 7th, Havana, Cuba. (G. M. Gilbert, Psychology Dept., Long Island Univ., Brooklyn 1, N.Y.)

26–31. American Assoc. for the Advancement of Science, annual, New York, N.Y. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5)

27–14. Bahamas Surgical Conf., Nassau. (B. L. Frank, P.O. Box 4037, Fort Lauderdale, Fla.)

27–29. Conference on Strong Interactions, Berkeley, Calif. (A. C. Helmholz, Dept. of Physics, Univ. of California, Berkeley.)

27–29. Northwest Scientific Assoc. and Idaho Acad. of Science, joint meeting, Moscow. (E. J. Larrison, Dept. of Biological Sciences, Univ. of Idaho, Moscow.)

28. Association for Education in International Business, St. Louis, Mo. (J. N. Behrman, Univ. of Delaware, Newark, Delaware)

28–30. American Economic Assoc., St. Louis, Mo. (J. W. Bell, Northwestern Univ., Evanston, Ill.)

28–30. Econometric Soc., St. Louis, Mo. (R. Ruggles, Dept. of Economics, Yale Univ., New Haven, Conn.)

28–29. Linguistic Soc. of America, annual, Hartford, Conn. (A. A. Hill, Box 7790, University Station, Austin 12, Tex.)

28–30. National Council of Teachers of Mathematics, Tempe, Arizona. (M. H. Ahrendt, 1201 16 St., NW, Washington 6, D.C.)

29–31. American Physical Soc., Berkeley, Calif. (K. Darrow, APS, Columbia Univ., 116 St. and Broadway, New York, N.Y.)

January

3–9. Indian Science Cong., 48th session, Roorkee (Uttar Pradesh), India. (General Secretary, ISC Assoc., 64 Dilkhusa St., Calcutta 17, India)

8–12. Thermoelectric Energy Conversion, symp., Dallas, Tex. (P. H. Klein, General Electric Co., Electronics Lab., Bldg. 3, Room 221, Electronics Park, Syracuse, N.Y.)

8–13. American Acad. of Orthopedic Surgeons, Miami Beach, Fla. (J. K. Hart, 116 South Michigan Ave., Chicago 3, Ill.)

8–14. Bahamas Conf. on Hypertension, Nassau. (I. M. Wechsler, P.O. Box 1454, Nassau)

8–14. International Conf. of Social Work, 10th, Rome. (Miss R. M. William, ICSW, 345 E. 46 St., Room 1012, New York 17)

9–11. Reliability and Quality Control, 7th natl. symp., Philadelphia, Pa. (R. L. Schwerin, ACF Electronics Div., ACF Industries, Inc., 11 Park Place, Paramus, N.J.)

9–12. White House Conf. on Aging, Washington, D.C. (Special Staff on Aging, Office of the Undersecretary, Dept. of Health, Education and Welfare, Washington 25)

9–13. Society of Automotive Engineers, annual, Detroit, Mich. (SAE, 485 Lexington Ave., New York 17)

10–11. Conference on Physics of Polymers, Bristol, England. (Organizing Secretary, Physical Soc., 1 Lowther Gardens, London, S.W.7)

16–18. American Astronautical Soc., annual, Dallas, Tex. (F. F. Martin, AAS, 304 S. Woodstock Dr., Haddonfield, N.J.)

16–19. Instrument Soc. of America, winter instrument-automation conf., St. Louis, Mo. (W. H. Kushnick, 313 Sixth Ave., Pittsburgh 22, Pa.)

22–28. Bahamas Serendipity Conf., 3rd, Nassau. (I. M. Wechsler, P.O. Box 1454, Nassau)

23–25. Institute of the Aeronautical Sciences, 29th annual, New York, N.Y. (Meetings Dept., IAS, 2 E. 64 St., New York 21)

24–27. American Mathematical Soc., 67th annual, Washington, D.C. (J. W. Green, Univ. of California, Los Angeles 24)

24–27. Society for Industrial and Applied Mathematics, Washington, D.C. (G. Kaskey, Remington Rand Univac, 1900 W. Allegheny Ave., Philadelphia, Pa.)

24–27. Society of Plastics Engineers, 17th annual conf., Washington, D.C. (T. A. Bissell, SPE, 65 Prospect St., Stamford, Conn.)

25–27. Mathematical Assoc. of America, annual, Washington, D.C. (H. L. Alder, Dept. of Mathematics, Univ. of California, Davis)

26–27. Western Spectroscopy Conf., 8th annual, Pacific Grove, Calif. (R. C. Hawes, Applied Physics Corp., 2724 S. Peck Rd., Monrovia, Calif.)

27–28. Royal College of Physicians and Surgeons, annual, Ottawa, Ontario, Canada. (T. J. Giles, 150 Metcalfe St., Ottawa)

28–30. Control of the Mind, symp., San Francisco, Calif. (Dept. of Continuing Education in Medicine, Univ. of California Medical Center, San Francisco 22)

29–3. American Inst. of Electrical Engineers, winter meeting, New York, N.Y. (E. C. Day, AIEE, Technical Operations Dept., 33 W. 39 St., New York 18)

30–3. Clinical Cong. of Abdominal Surgeons, Miami Beach, Fla. (B. F. Alfano, 663 Main St., Melrose 76, Mass.)

30–4. American Library Assoc., midwinter meeting. (Mrs. F. L. Spain, New York Public Library, 20 W. 53 St., New York, N.Y.)

(See issue of 18 November for comprehensive list)

Letters

Placebos for Relief of Pain

Beecher [*Science* **132**, 91 (1960)] presented a thesis that placebos are more effective for relieving pathological pain than for relieving experimental pain. The approach is very interesting, the data presented are clear-cut and convincing; however, I think that in interpretation one important factor is left out.

The data on pathological pain are based on observations in average, unsophisticated clinical patients. The subjects for the investigation of experimental pain are mostly medical or graduate students. As far as observation and interpretation of sensory phenomena are concerned, these students are surely in a different category from the average clinical patient. If, in addition, selection is limited to those volunteering for pain experiments, this puts the subjects in a very special class.

This was pointed out in several previous publications [*J. Appl. Physiol.* **8**, 630 (1956); *Science* **128**, 303 (1958)]. Beecher actually quotes from the second of these references, but he leaves out the main theme—the one indicating that the placebo effect becomes less pronounced with the greater ability of the subject to evaluate pain objectively.

I fully agree with Beecher's conclusions that placebos work on the anxiety component of pain and on anxiety-induced reflexes. However, I think that his own evidence indicates that this is largely due to differences in the psychological characteristics of the subjects—differences in degree of scientific understanding and in the ability to make objective evaluation.

FRED B. BENJAMIN
Republic Aviation Corporation, Farmingdale, New York

I am pleased, of course, that Benjamin found "the data presented . . . clear-cut and convincing" and that he "fully agree[s] with [my] conclusions that placebos work on the anxiety component of pain."

He is troubled, if I understand him, because the data on pathological pain are based upon the responses of "unsophisticated clinical patients," and those on experimental pain, on the responses of graduate students. He then makes a wholly unsupported statement; he says, "As far as observation and interpretation of sensory phenomena are concerned, these students are . . . in a different category from the average clinical patient."

But I am not at all sure that I know what Benjamin's real thesis is. He would not hold, presumably, that there are anatomical differences between the two groups, so he must believe that "conditioning" or "cultural" or economic differences make for different responses.

A great amount of effort has been devoted to demonstrating the presence or absence (according to the investigator's bias) of differences in pain threshold among Indians, Eskimos, Negroes, White subjects, North Europeans, South Europeans, men, women, the young, the aged, trained and untrained subjects, adapted and unadapted subjects, and so on. The enthusiast can "prove" about anything he wants to from this vast array of data [for references, see H. K. Beecher, *Measurement of Subjective Responses: Quantitative Effects of Drugs* (Oxford Univ. Press, 1959)]. It seems significant that no great differences have been uncovered and confirmed. Neither are the data as constant as others would like us to believe. Such differences as there are, are not great ones. In the study discussed in my report in *Science*, the difference between the two groups was tenfold. I am not at all certain how much familiarity Benjamin has with

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"clinical patients" in a large American medical center of today. Certainly they are unlike those of his native Germany and unlike those of Kashmir, India, where he practiced (dentistry). Since those days he has largely spent his time in physiological laboratories (according to the recent edition of *American Men of Science*). This is by no means to question his scientific status. This information is merely relevant to the question he raised. If Benjamin is trying to imply that the clinical patients are insensitive peasant types (if such exist), he is quite wrong. They are familiar with life's advantages and "sensitive" to them. While economic brackets are only one item in placing a man, I can point out that these television-owning, automobile-driving clinical patients are charged $27.00 per day for their beds and they actually pay 70 percent of this, or $18.90 per day. It is impossible for me to believe that the *tenfold* difference I showed could be explained by any such nebulous possibility as suggested by Benjamin.

There is an extraordinary constancy in the average response to morphine and to placebos, for example, if one deals with rather large groups of patients, notwithstanding diverse backgrounds. Houde and Wallenstein, studying chronic pain in cancer patients, found in 67 patients that 10 milligrams of morphine satisfactorily relieved ("relief" was carefully defined) 65 percent. Lasagna and Beecher found in groups of postoperative patients of a similar size in different years that 65.8 and 69.3 percent, respectively, were relieved ("relief" was carefully defined here also) by 10 milligrams of morphine. Houde and Wallenstein found that a placebo satisfactory relieved 42 percent of their patients; Lasagna and Beecher's figure was 39 percent. Here are remarkably similar results in groups whose past experience, present situation, and future are highly different. If the response to "observation and interpretation of sensory phenomenon" are as labile as Benjamin believes, one would have expected the lability to show up in a comparison of these two disparate groups. It did not.

The "active" drugs aspect of my report is pertinent to the present discussion. The universal effect of morphine in relieving more or less completely the pain of a wound, in graduate students as well as in all others (sophisticated or unsophisticated, it makes no difference), has been demonstrated. But some 15 groups of investigators have now utterly failed to demonstrate any dependable effectiveness of morphine on the experimentally produced pain threshold in (usually) sophisticated subjects. Here we find effectiveness in one instance and lack of it in the other, in groups of graduate students, depending on whether or not the pain was of pathological origin or was experimentally contrived. Benjamin's thesis breaks down here, for the effectiveness of the morphine was not determined by "differences in the psychological characteristics of the subjects—differences in degree of scientific understanding and in the ability to make objective evaluation."

To turn to another aspect of the problem, Javert and Hardy found that pain thresholds in clinical patients were normal, in comparison with thresholds in volunteers in their experimental studies, for a group of women before labor, during labor, and *post partum*. Many other data could be cited to indicate that the difference postulated by Benjamin has no support.

Benjamin speaks of the "ability of the subject to evaluate pain objectively." Pain is a subjective experience, subjectively evaluated. He refers again to "the ability to make objective evaluation," in his last sentence. I do not know what he means by these statements and therefore cannot discuss them.

One can erect a thousand straw men in this field, but if the tenfold difference I showed in a very large number of individuals is to be explained on any such vague basis as "psychological characteristics of the subjects—differences in degree of scientific understanding and in the ability to make objective evaluation" (whatever that last phrase means), there must be more evidence than Benjamin has yet produced to show, first, that these characteristics exist as determinants and, second, that they are relevant to the present study. I have indicated above several kinds of data to indicate that they are not of much importance, if any, in the present connection.

HENRY K. BEECHER
Harvard Medical School,
Massachusetts General Hospital,
Boston, Massachusetts

Sterilization of Interplanetary Vehicles

The article by Phillips and Hoffman [*Science* **132**, 991 (1960)] about the sterilization of interplanetary vehicles poses some interesting and difficult problems as regards one "component" that will be engaged in space travel—namely, man himself. Perhaps it is time that thought and investigation be given to the production of germ-free human beings.

JACK DE MENT
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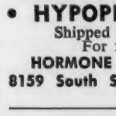

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